MEMOIRS

OF THE

NATIONAL ACADEMY OF SCIENCES.

Volume III.

PART 1.

1884.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1885.

MEMOIRS.

1. The Sufficiency of Terrestrial Rotation for the Deflection of Streams; by G. K. Gilbert.

2. On the Temperature of the Surface of the Moon; by Prof. S. P. Langley.

3. On a Method of Precisely Measuring the Vibratory Periods of Tuning-forks, and the Determination of the Laws of the Vibrations of Forks; with Special Reference of these Facts and Laws to the Action of a Simple Chronoscope; by Prof. Alfred M. Mayer.

4. The Baumé Hydrometers; by Prof. C. F. Chandler.

5. On Small Differences of Sensation; by Prof. C. S. Peirce and J. Jastrow.

6. Description of an Articulate of Doubtful Relationship, from the Tertiary Beds of Florissant, Colorado; by Dr. S. H. Scudder.

7. The Structure of the Columella Auris in the Pelycosauria; by Prof. E. D. Cope.

8. On the Structure of the Brain of the Sessile-eyed Crustacea. I. The Brain of Asellus and the Eyeless Form Cecidotea; by Prof. A. S. Packard.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

FIRST MEMOIR.

THE SUFFICIENCY OF TERRESTRIAL ROTATION FOR THE DEFLECTION OF STREAMS.

THE SUFFICIENCY OF TERRESTRIAL ROTATION FOR THE DEFLECTION OF STREAMS.

By G. K. Gilbert.

Read April 15, 1884.

It was long ago perceived that rivers of the northern hemisphere flowing to the north or to the south should by the rotation of the earth be thrown severally against their east or west banks. It is even many years since it was shown by Ferrel that these tendencies are but illustrations of a more general law, that all streams in the northern hemisphere are by terrestrial rotation pressed against their right banks, and all in the southern are pressed against their left banks, the degree of pressure being independent of the direction of flow. Yet the question of the sufficiency of the cause for the production of observable modifications in the topography of stream valleys is still an open one. A number of geologists have observed peculiarities of stream valleys which they referred to the operation of the law, while others, including myself, have looked in vain for phenominal evidence of its efficiency. Nevertheless it is my present purpose to maintain the sufficiency of the cause.

So far as I am aware, all those who have attempted to consider analytically the mode in which the lateral tendency arising from rotation should modify the channel or valley of a stream have reached the conclusion that no appreciable results can be produced, and for the most part their conclusions legitimately follow their premises.* My own different conclusion is based upon an essentially different analysis of the processes involved. In the celebrated discussion of the subject in the French Academy of Science, it was computed by Bertrand that a river flowing in 45° north latitude with a velocity of three meters per second exerts a pressure on its right bank of $\frac{1}{63539}$ of its weight, and he regarded this pressure as too small for consideration.† It has been pointed out by Henry Buff that the deflecting force, by combining with gravitation, gives the stream's surface a slight inclination toward the left bank, thereby increasing the depth of water near the right bank, and consequently increasing the velocity of the current at the right. To this increment of velocity he ascribed a certain erosive effect, but regarded it as less than that assignable to wind-waves on the same water surface. He therefore accorded a more important influence to the prevailing winds than to the rotation of the earth.‡ It has been held by others that the combination of the deflective force with gravitation is equivalent simply to a slight modification—so far as the stream is concerned—of the direction of gravitation; and that, the flood-plain of the stream having been adjusted normal to this modified direction of gravitational attraction, no other geological effects are produced. The last was my own view until I perceived the importance of certain considerations, to which I now proceed.

The form of cross-section of a stream flowing in a straight channel depends on the loading and unloading of detritus, and is essentially stable. It is evident that the form of the cross section

* I have recently become cognizant of a discussion by Baines to which this sentence does not apply. See note to the following page.

† Comptes rendus, XLIX, 1859, p. 658.

‡ Annalen der Chemie u. Pharmacie, IV Supp. I Band. Leipzig and Heidelberg, 1865–1866.

controls the distribution of velocities of current within its area, and that through the interactions of these velocities its parts are interdependent. Each element of its curve is so adjusted to the adjacent current and to the detrital load of the stream that it can neither be eroded nor receive a deposit, and the stability of the profile depends on the fact that an element not adjusted to the contiguous current and load becomes subject either to erosion or to deposition until an adjustment is reached. The distribution of velocities within the cross-section is symmetric, the swiftest threads of the current being in the center and the slowest adjacent to the banks.

If, now, curvature be introduced in the course of the channel, centrifugal force is developed. This centrifugal force is measured by the square of the velocity, and is therefore much greater for swift central threads of the current than for slow lateral threads. As pointed out by Thomson* and others, the central threads, tending more strongly toward the outer bank, displace the slower threads of that bank, and the symmetry of distribution of velocities is thus destroyed. In other words, the centrifugal force developed by curvature exercises a selective influence on velocities, and transfers the *locus* of maximum velocity from the center of the channel toward the outer bank. The conditions of symmetry in the profile of the cross-section are thus destroyed: the outer bank is eroded; a deposit is accumulated on the inner bank. Moreover, there is no compensating tendency to restore an equilibrium, for the erosion of the outer bank increases the sinuosity of the channel instead of rectifying it.

Curvature of course thus causes a stream to shift its channel laterally, and in this manner enlarge its valley. It is the most important condition of lateral corrasion.

As shown by Ferrel, the deflective force due to terrestrial rotation varies directly with the velocity of the stream. Therefore, it likewise has a selective influence on the velocities within the cross-section of the channel; and it likewise tends to produce erosion at one side and deposition at the other.† For given amounts of deflective force its selective power is not the same as that of the centrifugal force developed by curvature of course, for centrifugal force varies with the second power of the velocity, while the rotational deflective force varies only with the first power; but its selective power is of the same kind, and may be quantitatively compared. For the purpose of this comparison I will develop an equation:

Let F = deflective force, per unit of mass, due to rotation.
n = angular velocity of the earth's rotation.
v = velocity of stream.
λ = latitude of the locality.
ρ = radius of curvature of the stream's course.
f = the centrifugal force, per unit of mass, developed by such curvature.

Then

$$f = \frac{v^2}{\rho} \qquad (1)$$

and, from Ferrel,‡

$$F = 2vn \sin \lambda \qquad (2)$$

Let v_r = velocity of a rapid-flowing thread of the current, and
v_s = velocity of a slow-flowing thread of the current.

Represent by F_r, F_s, f_r, and f_s, the corresponding deflective forces due to rotation and curvature, then

$$F_r - F_s = (v_r - v_s) \times 2\,n \sin \lambda \qquad (3)$$

and

$$f_r - f_s = \frac{v_r^2 - v_s^2}{\rho} \qquad (4)$$

* Trans. Brit. Ass., 1876, Sections, p. 31.

† This proposition, which it is the prime object of the present paper to set forth and develop, was believed, at the time it was read, to be novel, but proves to have been anticipated by more than six years. In October, 1877, Mr. A. C. Baines read before the Philosophical Institute of Canterbury, New Zealand, a paper "On the influence of the earth's rotation on rivers," in which he arrived, by a very different route, at essentially the same conclusion. See Trans. N. Zeal. Inst., X, pp. 92–96.

‡ Ferrel's equation is given on page 29, volume 31 (second series), Am. Jour. Sci. Instead of the sine of the latitude, here substituted, it includes the cosine of the polar distance, which is, of course, equivalent.

$F_{,} - F$ evidently expresses the selective power due to rotation, and $f_{,} - f$ similarly expresses the selective power due to curvature. Where the curvature has a convexity to the right, these two influences conspire, and their resultant is deducible by addition. Where the curvature has a left-ward convexity, the influences are opposed, and their resultant is deducible by subtraction. [The terminology here and throughout the remainder of the paper is adjusted to the northern hemisphere exclusively.]

If we represent by R the joint selective power on curvatures of right-hand convexity and by L the joint selective power on curvatures of left-hand convexity, then we deduce, by simple combinations and transformations of equations (3) and (4),

$$\frac{R}{L} = \frac{r + r_{,} + 2\rho n \sin\lambda}{r + r_{,} - 2\rho n \sin\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

r, and $r_{,}$ may be the velocities of any two threads of current moving at different rates, but for purposes of convenience and simplification we now assume that they are symmetrically related to the mean velocity r: and introducing this relation in (5) we obtain

$$\frac{R}{L} = \frac{r + \rho n \sin\lambda}{r - \rho n \sin\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This equation expresses the ratio between the selective influences tending to determine the maximum velocity toward the right and left banks respectively of a meandering stream. Since these tendencies result in erosion, their ratio is a function of the tendency of a stream to erode its right bank as compared with its tendency to erode the left.

For the purpose of quantitative illustration, the Mississippi River will be considered. In its lower course the sharper bends have a radius of curvature, measured to the center of the channel, of about 8,000 feet. These curves, together with all other channel features, are determined by the water at its flood stage. It is therefore proper to consider in this connection the mean flood velocity. That was determined by Humphreys and Abbot to be, at Columbus, Ky., 8.4 feet per second.* The latitude of the locality is 37°. Giving these values to ρ, r, and λ, and substituting for n its numerical value, .000072924, we obtain from (6),

$$\frac{R}{L} = 1.087.$$

The selective tendency toward the right bank is therefore nearly 9 per cent. greater than toward the left.

With the elements of another stream it is probable that a very different result would be obtained; but this single example suffices to show that while the influence of rotation is small as compared to that of curvature, it is still of the same order of magnitude, and may reasonably be expected to modify the results of the more powerful agent. In the present state of hydraulic science it is impossible to define the quantitative relation between the tendency of swift threads of current toward a bank and the consequent erosion; but whatever that relation may be, I conceive that rotation is competent to produce appreciable results wherever those due to curvature are great.

It will be observed that the efficiency of rotation thus advocated is only in connection with, and as an adjunct to, lateral wear by means of curvature. There are two general cases, including a large share of all streams, to which the conclusion does not apply: (1) A stream which rapidly corrades the bottom of its channel does not notably corrade its banks; and in such case the effect of rotation should not be discoverable. (2) A stream engaged in the deposition of detritus, as on a delta or an alluvial fan, shifts its channel from side to side by a process entirely distinct from the one just described. It builds up its bed until it is higher than the adjacent plain, and then transfers its current bodily to a different course. Rotation has its share of influence in determining the direction of this transfer; and it thereby induces the stream to build its alluvial plain higher on the right than on the left; but, the difference of level having been established, the stream has

*Humphreys and Abbot, Report on Mississippi River, p. 595.

thereafter no more tendency to one side than the other. Deflective effects of rotation are therefore not to be sought in regions of alluvial deposition.

It may be remarked also that the tendency of a stream toward one bank or the other by reason of curvature and rotation is often overpowered by an opposite tendency due to obstructions. These include resisting members of the eroded terrane, and alluvial dams deposited at one bank or the other by tributaries.

A general curvature in the course of the valley through which the stream flows has the same tendency as does the curvature of a short bend, only in a less degree; and this tendency must in many instances nullify or conceal the results of rotation.

Visible examples of the work of rotation are therefore to be sought especially in streams which, with courses in the main direct, are slowly deepening their valleys by the excavation of homogeneous material. The best locality of which I have knowledge is one to which attention was called by Mr. Elias Lewis, in the American Journal of Science for February, 1877, and which has recently been visited at my request by Mr. I. C. Russell. The south side of Long Island is a plain of remarkable evenness, descending with gentle inclination from the morainic ridge of the interior to the Atlantic ocean. It is crossed by a great number of small streams, which have excavated shallow valleys in the homogeneous modified drift of the plain. Each of these little valleys is limited on the west or right side by a bluff from 10 to 20 feet high, while its gentle slope on the left side merges imperceptibly with the general plain. The stream in each case follows closely the bluff at the right. There seems to be no room for reasonable doubt that these peculiar features are, as believed by Mr. Lewis, the result of terrestrial rotation. As the streams carve their valleys deeper they are induced by rotation to excavate their right banks more than their left, gradually shifting their positions to the right, and maintaining stream cliffs on that side only.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

SECOND MEMOIR.

ON THE TEMPERATURE OF THE SURFACE OF THE MOON.

11

ON THE TEMPERATURE OF THE SURFACE OF THE MOON.

FROM RESEARCHES MADE AT THE ALLEGHENY OBSERVATORY BY S. P. LANGLEY, ASSISTED BY F. W. VERY AND J. E. KEELER.

Read October 17, 1884.

From the earliest ages it has been observed that the moon's rays bring us light, but no sensible heat. When, in the course of time, the phenomena of nature began to be subjected to more exact scrutiny, it was seen that in view of the very obvious brightness of the moon, the absence of heat in its rays was an anomalous circumstance, and in the last century Tschirnhausen, La Hire, and others, with the largest burning lenses or mirrors, and the most delicate thermometers of that time, attempted to obtain indications of heat, but without success. As apparatus improved in delicacy, it began to be noticed that (on the contrary) indications of actual cold were often obtained when the thermometer lay in the focus of burning mirrors which concentrated the rays of the moon on it: its concentrated heat, if any existed, being so nearly *nil*, as to be overbalanced by the increased radiation of the thermometer toward space or to the substance of the mirror itself. Other observers, like Howard,* fancied they obtained signs of heat with sensitive thermometers, but these were doubtless due to inexperience of the precautions necessary in eliminating the effect due to the radiations from the apparatus itself, radiations which may give delusive indications of marked lunar heat or cold, according (for instance) as the screen withdrawn be itself colder or warmer than the thermometer by some immeasurably small fraction of a degree. We can hardly overstate the probability of error in such a research in any hands but those educated to multiplied precautions, such as were used by Professor Forbes,† who, employing a lens by which the lunar heat was concentrated about 6,000 times, still obtained no certain evidence of heat. He was able, however, to conclude from this negative result that the warming effect of the full moon on the surface of the earth would at any rate not exceed $\frac{1}{300000}$ of a degree Centigrade.

The first satisfactory evidence of actual heat was obtained by Melloni,‡ who, with a polyzonal burning lens of one meter aperture and one meter focus, with the newly invented thermopile, in the clear air of Vesuvius, after due precautions against instrumental error, was enabled to announce that indications of heat had been obtained, though the effect was still all but immeasurably small. Prof. Piazzi Smyth,§ upon the Peak of Teneriffe, obtained also some apparent indications of heat, but all these measures, and a large number of others which I do not cite, including those of such skilled observers as Tyndall‖ and Huggins,¶ lead only to the conclusion that the moon's heat is so small that we can do little more than detect its existence, though M. Marie Davy** a little later obtains some apparent evidences of the change of heat with phase, indicating a direct effect of about $\frac{1}{100000}$ degree for the full moon, which he observes is about the one-fiftieth part of that found by Smyth. Such was the state of our experimental knowledge of the subject until the time of the observations of the present Earl of Rosse, which, as marking quite a new order of accuracy in lunar heat measurements, we reserve for a subsequent and more detailed discussion.

* American Journal of Science, II, p. 329 (1820).

† London Phil. Magazine, vi, p. 138 (1835).

‡ Comptes rendus, xxii, p. 541 (1846).

§ Report of the Teneriffe Astronomical Experiment, addressed to the Lords Commissioners of the Admiralty.

‖ Phil. Mag., IV Series, xxii (1861).

¶ Proc. Royal Society, vol. xvii (1869).

** Comptes rendus, lxix (1869).

The amount of heat received from the moon, and the dependent question as to the temperature of the lunar surface, are subjects of greater interest to us than might at first appear. They are even ones in which we may be said to have a material concern, for until we know the temperature which an airless planet* would attain in the sun's rays, we can have no accurate knowledge of the extent to which the atmosphere of our own planet contributes to its heat, nor of some of the most important conditions of our own existence. Those conditions are only lately becoming known, for it has hitherto been supposed that the temperature of the earth's surface was chiefly due directly to the radiation which it receives from the sun. It has been admitted, indeed, that the air acts to some extent in increasing the heat by hindering the radiation from the soil, but the manner and extent of this action have scarcely been, as it now appears, even surmised. Thus Sir John Herschel, distinguished as he is as a meteorologist as well as an observer, transferring to the moon conceptions drawn from the supposed state of things here, states that the temperature of the moon's surface in the lunar day must rise to 200° or 300° Fahr., and sink nearly as far below zero during the long lunar night, and the idea that the airless surface of the full moon must be intensely hot (in comparison with ordinary terrestrial temperatures) has since been generally accepted. Almost the only dissenting voice has been that of Mr. John Ericsson, who asserts that the lunar surface must, on the contrary, be intensely cold. In the present writer's opinion, the temperature supposed by Herschel, if it exist on the moon, will imply the presence of an atmosphere there; and if we can set aside the weight of traditional belief and preconceived impression the supposition that the surface of the moon (if absolutely airless) must be cold, even in full sunshine, is one which, however paradoxical it may appear, we are led to entertain by evidence at the command of everybody who can ascend high in our atmosphere. As we go up a mountain, we do not find the soil growing hotter, but colder, in the sunshine; and at great elevations, where the barometer is low, and we are partly approaching the conditions which must prevail on the moon (if airless), we find the surface covered with perpetual snow, even under the intenser solar blaze. The direct rays, indeed, are hotter, but the radiation from the soil is so far greater than below that, on the whole, with every upward step that diminishes the protection of our atmospheric envelope, the surface tends to grow colder. This is a matter of the most frequent observation. It is confirmed by the analogous experience of aeronauts, and it bears to my mind but one interpretation, that if we ascended still higher, until the air had been left altogether behind, we should find there regions of still intenser cold than any which we have experienced at the highest altitudes attainable by man. It has indeed been urged that the cold of high altitudes is largely due to the expansion of ascending air currents and to analogous causes, but our conclusions also rest on means which are independent of this hypothesis. In 1881 the expedition under the writer's charge to Mount Whitney, in the Sierra Nevadas, made many hundred actinometric observations at altitudes of from 3,000 to 15,000 feet upon the direct solar radiation, and its power to heat a thermometer bulb virtually removed from every disturbing influence, so that it was possible to estimate the result which would follow if the air between it and the sun were wholly withdrawn: the conclusion being that the temperature of the surface of the earth in full perpetual sunshine would, in the *entire* absence of its atmosphere, not rise much more than 18° C. over that of surrounding space. Now, the "temperature of space" must be conceded to be a vague and unsatisfactory term. To give a meaning to the expression we must ask what final temperature the earth's surface would attain were the sun's radiation and its own internal heat wholly cut off, and it were warmed only by radiations from other heavenly bodies, visible or invisible, or by the dynamic effects of meteorites, &c. Pouillet's conclusions are well known. The writer has reached much lower values, which he will not undertake to here state or explain. For the present purpose it is sufficient to say that, in his belief, the surface temperature of our planet, so far as it is due to direct solar radiation, would probably be such, that every liquid we know, and perhaps every gas, would exist only as a solid, though beneath the vertical rays of the sun.

It is hence almost wholly to our atmosphere and its capacity (by selective absorption) of storing the solar heat that, in the writer's view, we owe the high temperature which makes our exist-

* Certain discrepancies between observations on terrestial and lunar radiation suggest to us, however, the possibility of the existence of a minute lunar atmosphere, too small for recognition by the telescope.

ence on the earth's surface possible. But such conclusions are, it must be admitted, in contradiction not only to the statement already quoted from Sir John Herschel, but also to what has been regarded as direct experimental evidence as to the high temperature of the lunar surface obtained by the Earl of Rosse (that is, if we admit the moon to be absolutely airless, as Sir John Herschel assumes it; for it is not only possible, but even probable, that a gaseous envelope to the moon, too small to make its presence known to ordinary astronomical observation, would greatly raise the temperature of its surface, and the not impossible existence of such an envelope must be here borne in mind).

Lord Rosse's observations, in which, as we have already remarked, anything like quantitative measurement of the lunar heat has for the first time been attained, we shall proceed to examine in some detail.

ABSTRACT OF LORD ROSSE'S PAPERS ON THE RADIATION OF HEAT FROM THE MOON, WITH COMMENTS BY THE PRESENT WRITER.

[Proceedings Royal Society, XVI, p. 436 (1869).]

The object of the observations discussed in this paper is the determination of what proportions the lunar radiation contains of—

1. Heat coming from the interior of the moon, which will not vary with the phase.
2. Heat which falls from the sun on the moon's surface and is at once reflected regularly and irregularly.
3. Heat which, falling from the sun on the moon's surface, is absorbed, raises the temperature of the surface, and is afterward radiated as heat of low refrangibility.

The apparatus employed was a 3-foot reflecting telescope, with two small condensing mirrors and thermopiles. (A table of observations made at different phases of the moon is then given in the original paper.)

Assuming that the moon is a smooth sphere* without specular reflection, we may compute, from theory, the form of a curve, representing the amount of heat received from the moon as a function of her phase. This curve approximates to a sinuous form, having a greater curvature at the maximum or at full moon than at the minimum. The observations given in the table fall tolerably well on this curve, and therefore the increase and diminution of heat with the varying phase of the moon follow the same law as that of light. The heat classified under head (1) can have no existence in this case.

We may seek to determine the relative proportions of (2) and (3) present in the lunar radiation by experiments with thin plate-glass.

About 80 per cent. of the solar radiation probably passes through glass. From direct observations it was found that only 8 per cent. of the moon's rays was transmitted by the piece used in the experiment.

From this result, and the generally accepted value of the ratio of sunlight to moonlight, we may deduce the ratio of solar to lunar heat radiation. To do this Lord Rosse assumes† that all luminous rays are transmitted by glass and *all obscure rays are stopped.*

We have then (according to him)

Percentage of luminous rays in lunar radiation 8 per cent.
Percentage of luminous rays in solar radiation 80 per cent.
Ratio‡ of solar to lunar luminous rays = 800,000 : 1.
Ratio of total solar to total lunar radiation = 80,000 : 1.

The correctness of the value obtained for this ratio is confirmed by other considerations: in the first place, by direct measurement.§

The sun's rays were reduced by passing through a small aperture, and the deviation of the galvanometer connected with that previously found for full moon, by using the deviation produced by a vessel of hot water as a term of comparison. The ratio thus found was 89,819 : 1.

We may also find a value of this ratio from theoretical considerations. To do this he makes the following assumptions the basis of calculation:

---

* Zöllner has shown that the lunar surface reflects nearly like a flat disk, but the observations of Lord Rosse, given in the table, are subject to so great uncertainty, that they would fall equally well upon Zöllner's curve.

† This assumption is so far from the facts now known as to make the determination of the heat ratio depending upon it of little value, for it is now ascertained that in the solar-heat spectrum formed by a glass prism nearly two-thirds of the energy is represented by invisible rays.

‡ Lord Rosse assumes the largest known determination (800,000 to 1) as the most probable. There are, however, from eight to ten determinations by observers of repute, and all of them smaller; and if we take the mean of these, we have, as elsewhere shown, the approximate ratio 400,000 to 1. The agreement of the value 800,000 with the values obtained by the other methods given, on account of the falsity of the fundamental assumption of the latter, can only be regarded as a coincidence.

§ To the *experimental* determinations of the solar and lunar-heat ratio we make no objection, and they are probably as reliable as any others.

1. The quantity of heat leaving the moon at any instant may without much error be considered the same as that falling on it at that instant.

2. The absorptive power of our atmosphere is the same for lunar and solar heat.

3. As was assumed in a previous formula, the moon is a smooth sphere, not capable of reflecting heat regularly.

Deducing a formula for the amount of the moon's diffuse heat received by the earth and substituting in it the necessary values of the quantities entering, we find that the amount is 1/[illegible] of that received from the sun; a result which agrees well with the previous values.

The value of the galvanometer deflections was obtained by comparison with a vessel of hot water, which subtended the same angle at the thermopile as the large mirror. It was then found (the radiating power of the moon being supposed equal to that of the lampblack surface and the earth's atmosphere not to influence the result) that a deviation of 90 for full moon (about the average effect) appears to indicate an alteration of temperature through 500 Fahr. In deducing this result* allowance has been made for the *imperfect* absorption of the sun's rays by the lunar surface.

These observations must be regarded as merely preliminary, and the results may be subject to revision when more accurate measurements are obtained.

[Proceedings Royal Society, XIX, page 9, (1870)].

In the preceding paper it was shown that a large portion of the total lunar radiation consists of rays of low refrangibility emitted by the heated surface of the moon, which in the time of a complete revolution passes through a range of probably more than 500 Fahr. of temperature.

The ratio of the intensity of solar heat to lunar heat, as deduced from the observations, agreed well with values given by independent determinations.

Since the last communication, more accurate measurements have been made, with results substantially the same as those reached in 1869.

The glass used was found to transmit 87 per cent. of the sun's rays, 12 per cent. of the radiation from the moon, and 1.6 per cent. of that from a body at 130° Fahr.

Assuming† then that 92 per cent. of the luminous rays in either moonlight or sunlight is transmitted by glass, and 1.6 *per cent. of the obscure rays transmitted*, and taking 82,600 (found by direct experiment) for the heat ratio of solar to lunar rays, the resulting value of the light-ratio is 678300: 1, which agreeing well with the accepted value, shows that the heat ratio 82600 : 1 is very nearly correct.

In these experiments the quantity measured by the thermopile was the difference between the radiation from the circle of sky containing the moon's disk and that from a circle of sky of equal diameter not containing the moon's disk; no information in reference to the absolute temperature of either the moon or the sky results from them. The apparent temperature of the sky was found by comparisons with the radiation from blackened vessels of hot water at different temperatures to be from 17° to 32° Fahr. If the temperature of space be really as low as has been supposed, this result seems to indicate considerable opacity of our atmosphere for heat-rays of low refrangibility.

The observations made to determine the dependence of the heating power of the moon on her altitude, and the law of extinction of her rays in our atmosphere, are not very satisfactory on account of changes in the sky, which present great obstacles to measurements of this character.

The curve deduced in the first paper is given together with the later observations. As far as can be judged from so few and imperfect experiments, the maximum of heat seems to be a little after full moon.

[Proceedings Royal Society, XXI, p. 241, (1873).]

In this paper is given the result of recent and more careful observations made for the purpose of determining the dependence of the moon's heating power upon her altitude, the curve obtained being nearly but not quite the same as that found by Professor Seidel for the light of the stars, and showing a greater extinction of light than heat. By employing the table thus deduced, and introducing a correction for effect of change of distance of the sun, a more accurate phase curve was deduced, indicating a more rapid increase of the radiant heat on approaching full moon than was given by the formula previously employed, but still not so much as Professor Zöllner gives for the moon's light. At 79° zenith distance, the effect of atmospheric absorption is about one-tenth of the whole amount. Lord Rosse observes that this difference may be due to the fact that Seidel's light observations were made on the stars, not on the moon, and that it hence does not necessarily imply a different law for the extinction of light from that for heat.

From a series of simultaneous measurements of the moon's heat and light at intervals during the partial eclipse of November 14, 1872, it was found that the heat and light diminish nearly, if not quite proportionally, the minimum for both occurring at or very near the middle of the eclipse, when they were reduced to about half what they were before and after contact with the penumbra.

The probable error of a single set of 10 galvanometer readings at this time is given as about 19 per cent., or about 8.7 per cent. for the probable error of a night's observation: but Lord Rosse concludes that so large constant errors were probably present, that any increase in the number of sets was almost powerless to obtain a more reliable result.

*It will be seen by reference to a subsequent paper that this result is considered to be erroneous. What is meant by an allowance for imperfect absorption by the lunar surface is not clear.

†The assumptions we have italicized are little nearer the truth than those of the previous paper and the result entitled to little more weight.

[Nature, XVI, p. 438 (1877). Letter by Lord Rosse, replying to M. Raillard, who attributes the reddish tinge of the totally eclipsed moon to self luminosity due to the high temperature acquired under the sun's rays, and cites observations of Lord Rosse in support of his views.]

M. Raillard is mistaken in supposing that Lord Rosse estimated the temperature of the lunar surface at 500° Fahr. This was the range of temperature which a lamp-blackened vessel must have in order to exhibit effects similar to those of the moon in its different phases, deduced from early observations. More accurate observations (described in previous papers) show that this range is much more nearly 100° C., a large error having crept into the previous work. The observations made during the total lunar eclipse show that the diminution of heat kept pace with that of light. Probably not more than 5 per cent. of the heat acquired since new moon is retained till the middle of a total eclipse, although it has been shown that this heat has been absorbed by the lunar surface and reradiated. We must therefore fall back upon the usual explanation for the reddish color of the moon's surface during a total eclipse.

Urania I (1881).

On comparing the heat curves between new and full moon with that between full and new moon there appeared no conclusive evidence that the lunar surface required time to acquire the temperature due to the radiation falling on it. Accordingly, observations were made at the eclipses of November 14, 1872, and August 23, 1879, but the result of these was to show that the decline and subsequent increase of the heat took place as rapidly as that of the light.

[Nature, XXX, p. 589 (October 16, 1884). Description of observations made during the total lunar eclipse of October 4, 1884, by Otto Boeddicker, at the Earl of Rosse's observatory.]

The apparatus used was the same as that already described. Clouds prevented observations until 30 minutes before the beginning of the total phase, when the sky became exceptionally clear.

Two hundred and eleven readings of the galvanometer were taken, the time of exposure being 1 minute for each. (A curve representing these observations is given.) No observations were made during the total phase on account of the difficulty of judging when the image fell on the thermopile, but near the beginning and end of totality the effect was masked by the irregularities of the galvanometer, and was smaller than the probable error of observation. The minimum of heat seems to be *later* than that of light. As the moon emerged from the earth's shadow, so slowly did the readings of the galvanometer increase again that, about twenty minutes after the total phase was over, the almost entire absence of any effect led the observer to think that the small condensing mirrors must be covered with dew, which however was not the case.*

PRELIMINARY OBSERVATIONS AT ALLEGHENY.

OBSERVATIONS ON LUNAR HEAT.

The first measures of lunar heat at Allegheny were made on the evening of November 12, 1880,† more with a view to testing the sensitiveness of the then recently invented bolometer than for the sake of the measures themselves. The lunar rays were concentrated upon the face of the bolometer by means of the 13-inch equatorial of the observatory and a smaller convex lens near its focus, and an average deflection of 42 divisions of the galvanometer scale was obtained. The exposures were made by directing the telescope upon the moon after the galvanometer needle had come to rest, while the telescope was pointed at the neighboring sky.

On June 21, 1883, the bolometer and its adjuncts having been much improved in the interval, measurements of the lunar heat were resumed with apparatus better adapted to the purpose. The thick glass lenses, used in the previous experiments, absorb and reflect a large proportion of the already sufficiently minute amount of heat at disposal, and the rays absorbed are those whose presence or absence chiefly affects the conclusions to be drawn from the results of the observations. In the new experiments, therefore, the dioptric system for condensing the lunar rays was replaced by two silvered-glass mirrors, the absorption of silver for every heat ray of the spectrum having been determined by previous experiments here. This absorption, as is well known, affects chiefly the blue and violet rays, in which but a small proportion of the total energy resides. Our own observations show this absorption (by silver) to be very small and nearly constant throughout the infra-red. The sensitiveness of the bolometer, and its accuracy, enable us, as will be seen, to

*Although over one fourth of the moon's surface must have emerged from shadow at this time, it must be remembered that it was still covered by the earth's penumbra, so that the small heating effect is less surprising. If the moon parts with its acquired heat so soon as these eclipse observations seem to indicate, it is difficult to see how the maximum of heat could occur at an appreciable time after full moon. It is in any case hard to admit that this heat from the lunar surface, which the moon has been absorbing during many days of continuous sunshine, is parted with at once, the whole earthward surface of the planet cooling almost instantaneously.

†American Journal of Science, CXXI (March, 1881).

obtain with a comparatively small mirror, concentrating much less heat than that dealt with by the 3-foot reflector of Parsonstown, much more consistent indications.

The lunar rays, reflected from the 12-inch silvered mirror of a large siderostat, pass horizontally through an 8 inch circular aperture in the north wall of the observatory dark chamber, and fall upon a 10-inch concave silvered glass mirror of about 30 inches focus, mounted on a very solid tripod stand.* The bolometer, in a case specially designed for this work, is mounted on a sliding carriage directed toward the center of the mirror, so that it can be adjusted to such a distance as to bring the working face of the instrument into the plane of the lunar image. The bolometer case referred to has a series of circular diaphragms of various apertures so disposed as to protect the bolometer itself both from air currents and extraneous radiations, while just admitting the cone of rays from the concave mirror.

This mirror is inclined slightly to the incident rays, so that the bolometer can be placed a little to one side of the large aperture in the wall and not obstruct them. The lunar image can then be adjusted by means of the milled headed screws, by which the mirror is secured to the vertical plate, so as to fall truly on the bolometer strips before observation, and afterward in actual observation be carried on or off by a motion of the siderostat mirror outside, or by the moon's own motion in the heavens. In either case no screen is interposed, and no alteration in the relation of the radiating objects around takes place in reference to the bolometer, which experiences no changes, except those which come from its alternate exposure to the moon and the neighboring sky.

The lunar image is about 0.26 inch in diameter, and when properly directed is received by the working face of the bolometer, which it very nearly covers; hence it will be seen that (neglecting the absorption of the mirror, which is very small for invisible heat rays), remembering that only eight inches of the mirror's diameter is utilized, the intensity of the lunar heat was increased from 780 to 1,050 times, according as the distance of the moon from the earth varied. On a clear night with a full moon, and the galvanometer in its condition of then greatest sensitiveness, a deflection of 300 millimeter divisions of its scale could be obtained (in 1883), but toward the close of 1884, with still further improvements in the apparatus, this limit was much exceeded.

The "exposures" in later and adopted measures were only made, as we have said, by moving the image of the moon on and off the strips of the bolometer, by slightly inclining the siderostat mirror, thus simply replacing the image of the lunar surface by one of the adjacent sky. This was readily effected by means of a pulley on the azimuth screw of the siderostat from which a cord led into the building.

This method, we repeat, leaves the radiation from the apparatus itself unchanged by the introduction of the lunar heat, and avoids the disturbing influences which come from the interposition and withdrawal of a screen. Other methods, however, were tried in these earlier experiments, such as that of displacing the image by turning one of the screws of the concave mirror mount. This method, though sometimes yielding identical results, was found to be liable to errors, as was also that of the use of a screen, and in a still greater degree. It is not always easy to point out the exact nature of the error introduced in these delicate determinations, but we will give some of these preliminary observations to show the nature of the discrepancies presumably due to such methods of exposure.†

The character of the lunar energy, as compared to the solar, was first investigated as in Lord Rosse's experiments, by determining the relative transmissibility of the lunar and solar rays, as a whole, by certain pieces of glass, which were interposed in the path of the rays immediately in front of the bolometer case. Four pieces of glass were used for this purpose. The first was a disk 4.2 millimeters thick, of the same glass as the prism made by Adam Hilger, of London, used in a previous determination of wave-lengths in the infra red of the solar spectrum; the second was

* See Plate I, where M is the concave mirror; B, the bolometer; C, the cable connecting it with the galvanometer; G, the place at which the glass is interposed.

† The apparatus above described, as employed in 1883 and the summer of 1884, is substantially the same as that used in the later lunar heat measures, the chief improvements, since it was completed, having been made in the galvanometer and other electrical adjuncts of the bolometer. For a description of this instrument the reader is referred to earlier papers by the writer in the Proceedings of the American Academy of Arts and Sciences, xvi, 1881, and the American Journal of Science for March, 1881, and to details given later on in the present memoir.

a piece of plate glass ("A") 6.9 millimeters thick, apparently of English make; the third ("B") a piece of American plate glass, slightly greenish in hue, 6.6 millimeters thick, and the fourth a large pane of American window glass of good quality.

On July 9, 1883, October 4, 1884, and November 26, 1884, the diathermancy of these specimens of glass was determined for solar rays. The values obtained were:

	Per cent.
For the Hilger glass	86
For Glass A	86
For Glass B	77
For the large pane	76

the zenith distance of the sun being about 50°, or in the last case over 60°.

We have not as yet reduced these observations taken at somewhat different altitudes of the sun and moon to one common altitude, because it is sufficiently obvious from a comparison of the above figures without such a reduction with those indicating the absorption of the lunar heat by the same specimens of glass, that these absorptions are strikingly different in the two cases, and far more so than any difference in the altitudes of the bodies under consideration can account for. We will pass, then, to our preliminary observations on this unequal absorption before introducing small corrections which would be superfluous at this stage of the inquiry.

Preliminary measurements were made in pursuance of this system on the night of June 21, 1883, when all atmospheric conditions appeared to be favorable. A screen was, however, employed on this night to cut off the lunar rays, and the exposures made by withdrawing it (a method not capable of giving exact results).

If the glass be interposed while the lunar rays are falling on the bolometer, radiations from or to its substance will in general be confused with the effect to be studied, and further the radiations of low refrangibility from surrounding objects, such as those from the substance of the condensing mirror, will be cut off by it. It is always a condition of good observation, then, that the glass be placed in front of the bolometer and the instrument allowed to register its separate effect before the lunar rays are allowed to fall upon it; and this method has always been used.

It was found from the series of observations that the percentage of the total lunar heat transmitted by glass A was 70 per cent. and from a second series 54 per cent., giving a mean for glass A of 62 per cent.

For the percentage transmitted by glass B were obtained the values 60 per cent., 46 per cent., and 58 per cent., giving a mean of 55 per cent.

The discrepancies in these preliminary results were partly due to the above-mentioned erroneous method of exposure by withdrawal of a screen, but also to the difficulty of telling when the lunar image was exactly coincident with the bolometer face, since, when this was not the case, part of the heat was wasted and the deflection obtained too small, and the eye could not be safely brought into a position where the strips could be seen, since then the radiation from the observer's face gave a large deflection. This difficulty was subsequently overcome by placing a large sheet of glass behind the mirror, through which the observer could regard the bolometer strips without producing any disturbance of the galvanometer, the radiations from his face being completely intercepted by the glass. The conclusion, apparently resulting from the preliminary experiments where a screen was used, is that the specimens of glass appeared to transmit the greater part of the moon's rays; but that the apparent transmission of the glass should be greater when exposure is made by the withdrawal of a screen may be inferred from the following considerations. The screen is in general warmer than the external air, and if withdrawn while the bolometer was directed to the sky near the moon, a negative deflection of the galvanometer would be produced. When the screen is withdrawn while the bolometer is directed upon the moon, the heating effect of the latter is partly counteracted by the cooling effect of the sky or air between us and the moon, and the deflection obtained is smaller than that which would have been produced if the bolometer had been continuously exposed to the radiation from the sky. When, however, the glass is interposed, it forms a barrier to the interchange between outside objects of low temperature and the bolometer, and nearly the same deflection is obtained whether a screen is used or not. By the use

of a screen, therefore, the apparent transmission of lunar heat by glass is larger than it otherwise would be.*

These and other preliminary observations, then, are not used in the final results, but it has been thought worth while to refer to them to indicate some of the subtle causes of error which beset the commencement of such a research.

SUBSEQUENT OBSERVATIONS ON THE TRANSMISSIBILITY OF GLASS FOR LUNAR RAYS.

In October, 1884, these observations were again taken up and the transmissibility of the same pieces of glass redetermined.

The method of procedure was as follows: The apparatus being in adjustment and the galvanometer needle in a position of equilibrium, the lunar image was thrown on the bolometer by turning one of the concave-mirror screws, and the deflection of the galvanometer noted. Then the lunar image was thrown off the bolometer, and the new position of equilibrium noted, to which the galvanometer needle returned. This was generally slightly different from the original position. From these readings was obtained the effect of the uninterrupted lunar beam. A piece of glass was then interposed immediately in front of the bolometer case, and after the galvanometer needle had taken up a new position of equilibrium, caused by the alteration of conditions in regard to its thermal exchanges with the glass itself and with outside objects, the same operation was repeated, and the effect of the lunar ray obtained after it had suffered reflection at the surfaces and absorption in the substance of the glass. The following results, which are the means of repeated observations, were obtained by the writer under favorable conditions, except that exposures were made by touching the adjusting screw of the concave mirror, instead of that of the siderostat mirror. The image of the moon is thus replaced by that of the neighboring sky, but since the concave mirror is within the building, an alteration of the thermal conditions may be produced by an increased reflection of heat from the walls of the apartment in the mirror.

* To put this into symbolical form, let C = the amount of heat received from the walls of the bolometer case, s = the heat received from the screen, g = the heat received from the substance of the glass, S = the heat received from the sky, m_1 = the portion of lunar heat transmitted by glass, and m_2 = the portion of lunar heat absorbed by glass.

First step of observation.—The bolometer is exposed to the radiation from the screen, which is interposed between it and the sky. The heat received is then $C + s$.

Second step of observation.—The screen is then withdrawn and the bolometer exposed to the moon. The heat received is $C + m_1 + m_2 + S$.

The deflection of the galvanometer which is produced is due to the difference between these two amounts of heat or to $m_1 + m_2 + S - s$.

Third step of observation.—The screen is again interposed, and the plate of glass, completely cutting off the radiation from the screen, is placed in front of the bolometer, which then receives the amount of heat $C + g$.

Fourth step of observation.—The screen is now withdrawn and the bolometer again directed toward the moon. The heat from the sky, S, which consists entirely of radiations of long-wave length, is also completely cut off by the glass, and the amount of heat received by the bolometer is $C + m_2 + g$, so that the resulting deflection of the galvanometer is proportional to $(C + m_2 + g) - (C + g) = m_2$.

The ratio of the two deflections obtained as above is the apparent transmissibility of glass for lunar heat, and it is therefore

$$\frac{m_2}{m_1 + m_2 + S - s} = t_1$$

If observations had been made without the use of a screen, by moving the siderostat mirror, the transmissibility would have been

$$\frac{m_2}{m_1 + m_2} = t_2$$

The quantity of heat, S, depends upon the apparent temperature of the sky; that is, upon the temperature of the external air, and as this, except in an unusual combination of circumstances, is lower than the temperature inside the building, $S - s$ is negative, and consequently $t_1 > t_2$.

Measurements.

Radiation measured.	Deflection.	Amount transmitted.
Direct lunar heat	260	......
With Hilger glass interposed	70	.29
Direct heat	230	......
Hilger glass	69	.30
Direct heat	234	......
Glass B	77	.33
Direct heat	237	......
Glass B	64	.27
Direct heat	233	......
Glass A	74	.30
Direct heat	238	......
Glass A	62	.26
Direct heat	236	......

From these measurements it was concluded that the Hilger glass transmitted 29 per cent., the glass A 28 per cent., and the glass B 30 per cent. of the lunar radiation under the conditions in question.

A similar series of excellently accordant observations on the same evening by another observer placed the transmissibility of the Hilger glass at 27 per cent., of glass A at 27 per cent., and of glass B at 26 per cent.

The most striking feature about these results is their very fair agreement among themselves and yet their discordance with the previous measures of June 21, which also exhibit no striking discrepancies of an order equal to that existing between the two sets of measurements. It was therefore concluded that this discrepancy was in all probability chiefly due to purely local causes affecting the condition of the apparatus at the time of experiment, which we have reason to believe is in a large measure accounted for by the differing methods of exposure, and at the following lunation the measures were repeated, varying these conditions with especial regard to the following points: (1) Temperature of different portions of the apparatus, particularly of the large concave mirror; (2) temperature of the glass; and (3) place at which the glass was interposed. For the purpose of varying the latter condition, the large pane of window glass, already referred to, was fixed in a frame so fastened to the large flat of the siderostat that the lunar rays incident on the flat were first obliged to pass normally through the glass, without, however, being intercepted by it on their way to the aperture in the wall of the building.* The glass could be instantly withdrawn from the frame when desired and interposed immediately in front of the bolometer as in previous experiments, or elsewhere in the path of the rays. These later experiments were carried out with all due precautions, exposures being made only by inclining the siderostat mirror by means of its azimuth motion in the manner previously described. The transmission of the large pane for solar rays was determined with special care by a long series of observations, and was substantially the same as that given by former measures. The same apparent transmissibility was found for all positions of the glass, whether in the open air above the siderostat mirror or immediately in front of the bolometer case inside the building.

Experiments on the effect of varying the temperature of the glass and concave mirror by warming were only partially successful, since the immediate effect in either case was naturally that the progressive cooling of the heated object produced a violent "drift" of the galvanometer needle, so that measurements could only be resumed when the temperature had fallen nearly to that of the surrounding objects. Within this limited range of temperature, however, the transmission of the glass did not appear to vary as it presumably would have done had there been any change in the hygrometric condition of the surface of the plates or other disturbing cause.

* Since glass is athermanous to radiations from sources of such temperature as the bolometer strips or the walls of the room, its position with reference to these might (conceivably) affect the result. Although the mode of observation was calculated to eliminate any such effect, the experiment of placing the glass outside the building was therefore tried.

For the transmission of the large pane for lunar radiation were obtained the values:

	Per cent
Mean of observation of November 26	12.2
Mean of observation of December 2	14.5
Mean of observation of December 3	14.9
Mean of all	13.9

(the moon's zenith distance being about 45°.)

Observations on the variation of the coefficient of transmissibility of the lunar rays at different altitudes of the moon have been made at every opportunity, but the results are so dependent on fluctuations in our atmospheric conditions that they are at present only to be interpreted as showing that, if there be a difference in transmissibility of glass for lunar rays at different altitudes of the moon, this difference is not a conspicuous one.

Among the substances, whose power of transmitting the lunar radiation was tested, was a very thin disk of polished ebonite (thickness = 0.28 millimeters) through which the moon, when viewed with the naked eye, appeared of a dark-red color. It was found by experiment with the bolometer that this ebonite disk transmitted 6.9 per cent. of the moon's rays. Its transmission of the solar rays was 32.4 per cent. The transmission of the large pane for solar rays was also carefully redetermined, with the following results:

	Per cent.
Mean of observation of November 26	75.6
Mean of observation of December 3	75.1

These values are quite in accordance with those previously given.

All of the later and more careful observations show, therefore, that, whereas nearly 76 per cent. of the total apparent solar radiation is transmitted by the large pane of glass, only about 14 per cent. of the total apparent lunar radiation is transmitted.

PRELIMINARY PHOTOMETRIC OBSERVATIONS IN 1883.

The low transmissibility by glass which the lunar rays have been shown to possess by the experiments described in the first part of this paper is quite confirmatory of the experimental results of Lord Rosse, though not necessarily of his inferences from them. As we shall see, it may be partly accounted for by the supposition that the rays which reach us have suffered selective reflection at the surface of the moon. It is quite evident that, if selective absorption of heat take place, we ought to see it in the study of those heat rays which are also seen as "light." Moreover, as rays emitted from a source even of the temperature of boiling water can have nothing to do with vision, we shall not be liable to confound what we *see* with any effect due to radiation from the lunar soil, for what we thus observe must be due to reflected heat only (since "light" and "heat" are but names given to different manifestations of the same energy). Accordingly, if photo-spectrometric observations on homogeneous rays show a progressive selective reflection such that rays of low wave-length (such as are more absorbable by glass) are present in greater proportion after reflection from the moon than before, we shall undoubtedly be justified in concluding that the effect observed by Lord Rosse is in part, at any rate, due to this cause, and not *necessarily* to the presence of heat of low refrangibility radiated from the lunar soil. From the fact that the lunar light is not white like the sun's, but yellowish (Sir J. Herschel compares the moon's surface to that of sandstone rock), it was antecedently probable that such was the case. The fact has indeed been independently determined, but the writer was not familiar at this time with the work of others in this direction. The following apparatus, which was fitted up in June, 1883, was employed in the months of June, July, and October of that year for photometric comparisons of moon and sun light. It is not described more minutely because all the observations were afterward repeated with an improved form of it hereafter described.

The lunar beam, reflected from the siderostat mirror, passed into the dark room, fell on a silver-on-glass mirror of seven inches aperture and five feet focus which formed a lunar image on

the lower half of a slit, whence the light passed through a collimating lens, and fell upon a large Rutherfurd grating of 17,296 lines to the inch, whose diffracted rays were viewed by an observing telescope. The inclination of the grating was determined by a graduated circle and vernier, so that by use of the customary formula the exact wave length of the color or line in the center of the field could be computed. On the upper part of the slit was a prism of total reflection which brought in the rays from an Argand burner arranged to slide at right angles to the axis of the collimating telescope along a graduated scale. The amount of gas supplied to the burner was controlled by a meter. Accordingly, a spectrum from a flame of standard and constant brightness was formed by the same grating in juxtaposition to the lunar spectrum immediately under it in the apparent field and viewed by the same eye-piece. The lamp was now withdrawn or approached until some particular wave-length (*e. g.*, the yellow about $0^{\mu}.6$) was judged to be of like strength in either spectrum. Under these conditions if the grating was rotated so as to bring in more of the blue end of both spectra, the moonlight spectrum grew constantly brighter relative to that of the gas light, so that it was necessary to strengthen the latter light to re-establish equality. The field was limited by a diaphragm to a narrow strip of both spectra, whose edges were brought as closely into juxtaposition as possible, and numerous series of comparisons were taken throughout the visible spectrum, which after the requisite corrections and reductions gave the relative intensity of the lunar spectrum in each part to that of the gas. The same apparatus was used for the solar comparison in the same way, except that the stronger sunlight was allowed to enter through a smaller aperture and was diffused, instead of concentrated, by being allowed to fall on a convex silvered mirror. It was evident that the proportion of blue in the sunlight was greater than in the moonlight, as the following results show.

Observations of June 20th to 22d.

(Corrections for altitude have not been applied.)

For wave-length μ .471, sunlight 2,183,000 times moonlight.
For wave-length .581, sunlight 232,140 times moonlight.
For wave-length .625, sunlight 30,600 times moonlight.

These comparatively rough preliminary values are not believed to have any great quantitative accuracy, but they at least show clearly that there is selective absorption of light (and hence of heat) throughout the visible lunar spectrum, of such a kind that the rays less transmissible by glass will be found (so far as our investigation extends) in greater proportion in moon heat than in sun heat, irrespective of any question as to sensible radiation from the lunar soil. It was evident that the photometric method was liable to error considerable enough to make very considerable discrepancies between the work of careful observers, and the general results only are given above, because the work of 1883 was supplemented by a more careful series of observations in 1884, which we now proceed to give in detail.

GENERAL CONSIDERATIONS.

Zöllner has shown that, owing to the irregularities of its surface, the full moon does not reflect as a smooth sphere would do, but very nearly as a flat disk of like reflecting power, and filling the same angle. Such a disk, if it presented, as seen from the earth, the mean semidiameter of 15′ 35″, and if it diffused all the solar energy which fell on it,* would send to us $\frac{1}{97300}$ of what the sun does, which is the portion of the solar energy which we should receive from such a moon, reflecting perfectly (not specularly, but in all directions) all the solar energy which fell on it. The moon, however, is far from being a perfect reflector. The color of its surface is comparable (as we recall) to that of sandstone rock, and hence it must reflect selectively, and, as far as we can see, in such a manner that the longer wave-lengths are in larger proportion in the reflected than in the original solar beam, in which, roughly speaking, the luminous energy is about one half of the non-luminous or dark heat. Since the moon then only imperfectly re-

* Considerable difference may exist even in values obtained from such geometrical considerations. Thus Lambert's formula gives the number [illegible], which is nearly that used by Lord Rosse; and George P. Bond (Memoirs of American Academy, vol. VIII) uses the value [illegible], where we have taken $\frac{1}{97300}$.

flects or diffuses, part of the solar energy must be absorbed and re-radiated as dark heat. We make no doubt, then, that the lunar soil radiates heat toward space. The real questions at issue are "At what temperature does it so radiate?" "Can we have any experimental knowledge of such dark heat radiation at the earth's surface?" If we suppose, for instance, the lunar soil to be heated by the sun 50° C. above the temperature of surrounding space, then in the case of this very considerable supposed heating effect, the moon's surface will remain far below zero in the sunshine, and though it may be said in one sense to radiate heat to the earth, yet since it is in this case below the mean temperature of the earth's surface, we should obtain no sensible heat from it, even were our atmosphere altogether absent, while the actual presence of our atmosphere, athermanous, as it is generally believed to be to such radiations, would render their determination hopeless. Whether the moon be a perfectly diffusive body or the actually imperfectly diffusive one, we get the same amount of heat from it; for it will finally attain a condition of heat equilibrium in which it will send away as much as it receives. In the first hypothesis, what it sends away will be purely reflected or diffused energy, of wave-length corresponding to what it has received from the sun; in the second hypothesis, the radiant energy will be partly reflected, and partly that of much lower wave length emitted by the soil. The second hypothesis, doubtless is the true one; but the question before us is, "Is this re-radiated heat sensible?"

From the fact that the lunar energy appears less transmissible by glass than the solar it has been assumed that the entire effect is due necessarily to a large heat radiation from the lunar soil, which our atmosphere transmits and the glass stops. Before we accept this hypothesis we must repeat that it does not necessarily imply this, for we have only to suppose the selective reflection exercised on the solar rays at the surface of the moon to be such as to send us in the reflected rays an undue proportion of those which glass absorbs, to account, at least in part, for the observed effect. We will pass, therefore, to a series of observations which show more clearly than any yet given that a selective absorption of such a character does actually take place.

PHOTOMETRIC OBSERVATIONS IN 1884.

Comparative photometric measures of the intensities of solar and lunar rays are of importance, as we have seen, to our heat determinations, and especially is this the case when such measures are combined with others (to be shortly given) of the comparative amounts of heat received from the sun and moon. The complete knowledge desirable would tell us of the special ratio of each separate heat or light ray, but even a knowledge of the ratio of the total sunlight to moonlight and the total sunheat to moonheat will be valuable. If, for instance, it were found by purely optical means that the intensity of sunlight was m times that of moonlight, and by an instrument like the thermopile or bolometer, in which the registered effect of the radiation is proportional to the amount of energy which resides in it, that the heat received from the sun was only n times that from the moon, even such a result would enable us to draw some inference as to the general character of the lunar energy, and hence of the conditions of temperature of the moon's surface. For, in the case above stated (supposing $m > n$), the given relation between the light and heat ratios could be explained only on the supposition that the energy was distributed differently in the two spectra, a larger portion of that residing in the lunar rays being unable to produce any physiological effect when received upon the retina or incapable of being interpreted as light, and hence that the surface of the moon had either selectively reflected the solar rays or had added to them radiations from its own substance indicative of a considerable individual temperature. We have seen, however, that a difference in the direction of the above supposition is to be expected from the effects of selective reflection at the moon's surface.

The chief objection to such a comparison between the light and heat ratios of the sun and moon is the difficulty of making the necessary measurements with the requisite degree of accuracy; so that, unless the difference were extreme, it would be masked by the effects of the errors of observation. The photometric comparisons are generally made with the aid of an artificial source of light of intermediate brightness, which at once introduces a considerable degree of uncertainty into the problem on account of its variations in intensity. Differences in altitude and changes in the state of the atmosphere have also great influence upon the result; and it has been shown by the writer how great is the difficulty of making certain allowance for the effect of these

unequal circumstances by processes of mathematical computation. Even for the relative total brightness of the sun and moon very discrepant results have been reached, which may be best exhibited in the form of a table of the principal determinations.

300,000 : 1	(Lambert.)
400,000 : 1	(Lambert, allowance made for various errors.)
300,000 : 1	(Bouguet, Essai d'optique, &c.)
801,000 : 1	(Wollaston, Phil. Trans. (1829), vol. 8.)
480,000 : 1	(Bond, Memoirs American Academy, vol. 9.)
618,000 : 1	(Zöllner, Photometrische Untersuchungen, p. 105.)
350,000 : 1	(W. H. Pickering, Pr. Amer. Academy, 1880.)
70,000 : 1	(Sir William Thomson.)

There has been no essential improvement in such photometric processes as are here in question since the early measures by Bouguer and Lambert. Zöllner's are perhaps made with more care than others, but giving all these values equal weights, we have 405,800 : 1 as the mean ratio. It is sufficiently evident that the limits of error are here wide, and we shall adopt 400,000 to 1 as the most probable value.

Recurring now to the comparison of separate spectral rays in sunlight and moonlight, we find that investigations have been independently made by two competent observers.

In those of W. H. Pickering,* in which light from various sources was compared with that from a standard Argand gas-burner at four different parts of the spectrum, there is a very great preponderance of violet in the solar rays as compared to the lunar. It is possible that the difference is too great, but we have already remarked upon the extreme difficulty of real accuracy in such determinations, and our own earlier observations are of a like order of discrepancy.

Dr. H. C. Vogel,† compared, by means of a spectro-photometer, in which a petroleum lamp served as a standard, moonlight and sunlight which had been reflected from various kinds of rock. As a result he found that a selective absorption of the more refrangible rays of the spectrum took place on reflection of the solar rays by the surface of the moon, although not sufficiently pronounced to indicate any very decided color in the substance of which it is composed. The moonlight agreed best with sunlight reflected from yellowish gray sandstone.

The spectro-photometer used at Allegheny in the later observations (in 1884)‡ to determine the amount of the selective reflection under consideration was the result of the experience obtained in 1883, and at the same time not dissimilar in principle to those employed in the researches of Vogel and Pickering, the brilliancy of the two spectra being compared at different points by means of an artificial source of light of supposed constant intensity. This artificial source was a kerosene lamp in which the oil was kept at a constant level, with Argand burner, and screens so placed before the glass chimney as to limit the effective part of the flame to a cylindrical portion 10 millimeters high, taken where it was brightest. The lamp was trimmed and cleaned before each set of observations, and although the constancy of its light seemed to be all that was desired, the quality was so different from that of either of the two heavenly bodies to be compared that the accuracy of the observations was not so great as would have been obtained if a source like the electric light, for example, had been used.

In order to carry out the measurements, the intensity of the sunlight had to be diminished, and that of the moonlight increased until they were both comparable with the standard. In doing this no attempt was made to determine the amount of diminution or increase, although a rough approximation to this is possible, but as only relative brilliancies in different parts of the two spectra were desired, attention was mainly paid to securing a convenient intensity in the light to be compared.

Plate 2 represents the arrangement of the apparatus. The light, reflected horizontally by the 12-inch silvered mirror of the siderostat, enters the dark room by an aperture, A, in the north wall. Here, if it is sunlight which is being compared, all is stopped except what passes through a small cir-

* Proc. Amer. Academy, 1880, p. 236.

† Monatsberichte d. Konigl. Akademie d. Wissenschaft z. Berlin, Oct. 21, 1880.

‡ These observations were conducted by Mr. J. E. Keeler of this observatory.

cular aperture 4.86 millimeters in diameter, in the center of a cap covering the object-glass of a small telescope, D, of about 520 millimeters focus. On leaving the eye-piece of this telescope the sunlight is spread out into a diverging cone of rays, which at the distance of the photometer slit S, 2610 millimeters beyond the eye-piece, has a diameter of 652 millimeters. Its intensity has therefore been weakened about 18,200 times (independently of the absorption of the glass, which is not a a factor in our qualitative determination).

S is the slit of a grating spectroscope. The collimator C has a focal length of 1254 millimeters and an aperture of 57 millimeters. The observing telescope T is much shorter, having a focal length of but 400 millimeters (in order that the head of the observer may not interfere), and is set at a fixed angle of about 49° to the collimator. A holder within the case at G carries a flat Rowland grating with a ruled surface 51.6 by 35.0 millimeters, and with the number of lines per millimeter equal to 568.4. This grating, which gives very brilliant and very perfect spectra, was used in such a position that the normal to its surface fell between the two telescopes, the comparisons being made in the brighter first spectrum. Its angular position is indicated by a divided circle and vernier, reading to minutes on the outside of the case.

The lower part of the photometer slit is covered by a totally reflecting prism, P, which cuts off the sunlight entering there and substitutes for it the light from the standard lamp, L. Two spectra in close juxtaposition are therefore seen in the eye-piece of T, the upper belonging to the lamp and the lower to the sun. By means of a 2-millimeter blackened cardboard slit in the common focus of the object-glass and eye-piece, the range of wave-lengths included in the field of view was limited to $0^{\mu}.0048$, or about eight times the interval between the D lines.

The lamp has already been partially described. It was fastened to a slider, which could be drawn by the observer to and fro along a graduated scale, at right angles to the collimator so as to approach or recede from the slit, by pulling a cord. A heavy screen which hung down to the level of the photometer scale concealed the lamp from the observer, who was thus unaware of its position while making a measurement (except from the appearance of its spectrum in the eye-piece of the telescope) until the index had been read, and thus any bias resulting from a preconceived opinion as to the proper position of the lamp was avoided. The range of the scale was 20 decimeters, and its zero-point was so adjusted that the reading of the index of the lamp-carriage was the distance of the center of the flame from the slit of the photometer. On account of the great difference in the quality of the lights compared, the range of the scale proved to be insufficient, and the wheel photometer, an instrument presently to be described, was used to diminish the more intense light by a given ratio.

When moonlight, instead of sunlight, was compared with the standard, the diminishing telescope was removed, and a telescope of 1,054 millimeters focus and 77 millimeters aperture, with the eye-piece removed, was placed on the axis of the beam from the siderostat, so as to form an image of the moon on the upper half of the photometer slit.

The wheel-photometer, referred to above, consists of two circular disks of sheet-zinc about twenty inches in diameter, each pierced near the circumference by eighteen radial apertures separated by spaces of the same width. The two disks may be rotated past each other with considerable friction, enough to hold them firmly in relative position, and are held by an axis passing through their centers, by means of which, and a multiplying wheel connected with it, they may be rotated in a vertical plane with great velocity as a single wheel. If they are adjusted to coincide, and rotated in front of a source of light, they diminish its brilliancy one-half, although, on account of the persistency of vision, the eye does not perceive any flickering or unsteadiness caused by the interruptions of the spokes. A graduated arc is attached to one of the disks, and an index to the other, so that the apertures may be adjusted to any width from the full opening down to zero. Thus the intensity of a luminous source may be diminished to any fraction less than one-half of its original value.

On looking into the eye-piece of this apparatus (Plate 2) two nearly square patches of light were seen, the lower belonging to the sun or moon, and the upper to the lamp. The color of the light would depend, of course, upon the position of the grating. The observations were made

by sliding the lamp along the scale, by means of its cord, until these two squares of light were of equal intensity. Then, if the intensity of the standard is known for all points of the scale, we obtain the intensity of the sunlight or moonlight at that part of the spectrum. If the wheel-photometer was used, a proper factor must be introduced to give the degree of diminution caused by it. Eight points in the spectrum at which comparisons were to be made, which we may roughly designate by their approximate colors, were selected. Their wave lengths, and the settings of the grating circle, together with those for several of the Frauenhofer lines, are given in the annexed table.

Point.	Color.	Line.	Setting.	Wave-length.
			° ′	μ
1	Deep red......	*B*...	84 5	0.687
....	....	*C*...	84 35	0.656
2	Bright red.....	......	84 47	0.649
3	Orange........	......	85 40	0.599
.....	....	*D*...	85 49	0.589
4	Yellow	......	85 52	0.586
5	Green	(*b*)..	87 8	0.518
6	Blue	*F* ..	87 42	0.486
7	Bright violet ..	......	88 0	0.470
8	Deep violet....	......	89 0	0.415

A table giving the intensity of the illumination in the observing telescope, obtained from the photometer lamp for each decimeter of the lamp-scale, was next constructed from data obtained by observation. The assumption which has been made in similar photometric measures that the intensity of the illumination is inversely proportional to the square of the distance of the lamp-flame from the slit, leads to results which may be considerably in error, particularly if some of the observations were made when this distance was small. The reasons for this are various. If, starting with the lamp at the end of its scale, we slide it gradually forward toward the slit, the intensity of the light in the observing telescope will increase gradually until the aperture of the collimator is filled, and then on closer approach the intensity no longer increases but remains constant, whereas by the law of inverse squares it should increase from a certain value up to infinity at the slit. In the apparatus used in these experiments this constancy of illumination began at about 2 or 3 decimeters, and measurements made with a smaller scale reading than 5 decimeters were avoided as much as possible. On account of the small proportion of blue and violet rays in the lamp-light, however, it was sometimes necessary to make the comparisons in the upper end of the spectrum with the lamp so near the slit that the value of its light intensity was subject to considerable uncertainty, and it is for this reason that the great difference in quality between the standard and the lights to be compared is so prejudicial to the accuracy of the observations.

It was preferred, in making the measurements, to diminish as much as possible the violet of the sunlight or moonlight by means of the wheel-photometer, thus enabling the comparison to be made with the lamp at a greater distance from the photometer slit.

The edges of the lamp flame are considerably more brilliant than the central portions. When the lamp is near the extremity of its scale, its effective brilliancy is the average of that of all its parts, but when brought up close to the slit, the effective rays are those from the central portions only. From both this and the foregoing reason, the decrease in the brilliancy of the light in the observing telescope as the lamp is moved away from the slit is less rapid than that required by the law of inverse squares.

The law actually followed was determined empirically by means of the wheel-photometer. A second kerosene lamp with Argand burner, quite similar to the standard lamp, was placed directly in front of the slit, at such a distance that when matched by the standard lamp the scale reading of the latter was a little less than 5 decimeters. Having determined this reading by taking the mean of five settings, the wheel-photometer with its index set to 10 (or with its apertures open to their full width), was interposed between the auxiliary lamp and the slit, cutting down the brilliancy of the direct light to one-half: and the new position of the photometer lamp, when matched

with the diminished auxiliary, was determined as before by five settings. The index of the wheel was then set to 9, reducing the intensity of the direct light to nine-twentieths, and so on until the reduction amounted to two-twentieths, when the limit of the lamp-scale was reached.

The following observations were made on November 5, 1884, each position of the photometer lamp being the mean of five independent settings, which sometimes, though very rarely, differed from each other by as much as 1 decimeter, the usual variation being from 1 to 5 centimeters. The comparisons were made in the yellow, experience having shown that equality was most accurately judged of in that color.

Setting of wheel-photometer.	Intensity.	Reading of lamp scale.
		Decimeters.
No wheel	1.00	4.91
Wheel index at 10	.50	7.60
Wheel index at 9	.45	8.06
Wheel index at 8	.40	8.34
Wheel index at 7	.35	8.90
Wheel index at 6	.30	9.74
Wheel index at 5	.25	10.84
Wheel index at 4	.20	12.34
Wheel index at 3	.15	14.76
Wheel index at 2	.10	18.34
No wheel	1.00	4.70

These observations, when plotted, give points which fall very nearly on a smooth curve. From this curve we may then take the intensity corresponding to even decimeters on the lamp scale, the unit of intensity being one-twentieth of the intensity of the auxiliary lamp. Finally, we may express the intensity in terms of another purely arbitrary unit; namely, that of the standard lamp at a distance of 5 decimeters from the slit. We thus obtain the following table. The last column is the adopted value of the lamp intensity at each division of the scale, obtained by taking the mean of this and another similar set of observations.

Scale reading.	Intensity in twentieths.	Intensity.	Adopted intensity.	Scale reading.	Intensity in twentieths.	Intensity.	Adopted intensity.
Decimeters.				*Decimeters.*			
5	19.1	1.00	1.00	13	3.7	.19	.20
6	15.2	.80	.81	14	3.3	.17	.18
7	11.9	.62	.63	15	2.9	.15	.16
8	9.1	.48	.56	16	2.6	.14	.14
9	6.8	.36	.38	17	2.3	.12	.12
10	5.7	.30	.32	18	2.1	.11	.11
11	4.9	.26	.27	19	1.9	.10	.10
12	4.2	.22	.23	20	.7	.09	.09

Plate 3 is a curve representing the intensity of the photometer lamp as a function of the scale reading, as determined by the experiments, and also the curve (dotted), on the assumption that the intensity varies inversely as the square of the distance from the slit. The less rapid decrease of intensity by the actual law is apparent. The unit of intensity in the last column, namely, that of the photometer lamp, at 5 decimeters from the slit, will be used throughout *for all colors*, no matter what their relative proportions in the lamp-light may be. The observations made on the moon on November 2, 1884, and those on the sun November 7, 1884, are given in full below. The observations of November 2, 1884, on moonlight, were made between the hours of 10 and 11 p. m.

Photometric observations on moonlight.

[Observer J. E. Keeler. All conditions favorable. The sky "at first slightly hazy, gradually becoming perfectly clear." At the time (10 h. 30 m.) the moon's hour angle was 1 h. 3 m. and her declination + 10°, corresponding to a zenith distance of 34° and air-mass M = 1.21.]

Setting.	Color.	Reading of lamp-scale.					Mean.	Remarks.
° ′								
84 5	Deep red	12.2	13.0	12.1	10.9	11.9	12.0	Wheel before lamp, index at 3.
84 47	Bright red	11.4	10.4	10.7	10.1	9.9	10.5	Do.
85 40	Orange	8.4	7.6	7.9	7.7	8.0	7.9	Do.
85 52	Yellow	9.7	8.6	8.8	8.7	8.8	8.9	Wheel index at 5.
87 8	Green	6.2	5.9	6.3	5.6	5.6	5.9	Do.
87 8	......do	11.6	12.4	12.1	11.7	12.2	12.0	No wheel.
87 42	Blue	8.9	8.5	8.8	8.2	8.1	8.6	Do.
88 0	Bright violet	6.7	6.6	6.9	6.4	6.1	6.5	Do.
89 0	Deep violet	3.4	3.0	2.4	2.3	2.9	2.8	No wheel; faint.

These observations are reduced with the aid of the table of lamp-light intensities on page 28. In the following table the last column contains the intensity of moonlight in different parts of the spectrum, as compared with the standard lamp:

Wave-length.	Mean lamp setting.	Tabular intensity.	How modified by wheel.	Actual intensity.
μ				
0.687	12.0	.230	Reduced to $\frac{1}{6}$	.035
0.649	10.5	.295	......do	.044
0.599	7.9	.513	......do	.077
0.586	8.9	.392	Reduced to $\frac{1}{4}$	.098
0.518	5.9	.829	......do	.207
0.518	12.0	.230	Not modified	.230
0.486	8.6	.428	......do	.428
0.470	6.5	.720	......do	.720
0.445	2.8	....	......do	....

In the observations of November 7 the sun was near the meridian (exact time not noted), and his declination being —16°, his zenith distance was about 56°, corresponding to an air-mass of M=1.79. The condition of the apparatus was the same as in the previous measures. The sky was a "fair hazy blue."

Photometric observations on sunlight.

Setting.	Color.	Reading of lamp-scale.					Mean.	Remarks.
84 5	Deep red	12.2	13.1	12.0	11.2	11.2	11.7	Wheel before lamp, index at 5.
84 47	Bright red	9.0	8.7	8.6	8.0	8.8	8.6	Do.
85 40	Orange	15.4	15.7	16.2	14.8	15.0	15.4	No wheel.
85 52	Yellow	13.5	11.7	12.9	12.7	11.6	12.5	Do.
87 8	Green	6.0	6.2	6.2	6.2	6.5	6.2	Do.
87 42	Blue	8.9	9.5	9.6	9.7	9.6	9.5	Wheel before sun, index at 5.
89 0	Bright violet	8.2	8.0	8.6	8.2	8.4	8.3	Do.
89 0	Deep violet	6.0	5.4	5.5	6.0	5.6	5.6	Do.

These observations are reduced in the same way as those given in the first example. It is evident that introducing the wheel photometer in the path of the sunlight increases the value of

the light intensity at a given division of the lamp-scale by the same factor that it is diminished when the wheel is interposed between the lamp and the slit.

Wave-length.	Mean lamp setting.	Tabular intensity.	How modified by wheel.	Actual intensity.
μ				
0.687	11.7	.242	Reduced to $\frac{1}{4}$	.061
0.649	8.6	.428	....do	.107
0.599	15.4	.152	Not modified	.152
0.586	12.5	.215	....do	.215
0.518	6.2	.774	....do	.774
0.486	9.5	.350	Increased to 4	1.400
0.470	8.3	.464	....do	1.856
0.415	5.6	.886	....do	3.544

Two more such complete sets of observations were made under favorable circumstances, one on the sun on November 1, and one on the moon on October 31. Other observations made under disadvantageous circumstances, such as a hazy or smoky sky, were rejected.

The differences, which are sometimes considerable, between those results of these observations which should be identical, are due to errors of observation, as well as to different conditions of the atmosphere at the times of observations, differences in altitude of the heavenly bodies observed, and variations in the intensity and quality of the light from the photometer lamp. Since the effects of these sources of error, with, perhaps, the exception of that due to difference in altitude, do not allow of computation, the best we can do is to regard them as made under perfectly similar circumstances and combine them accordingly. We shall then at least know, from a consideration of the general effect of the actual differences in circumstances, in which direction the error of the combination lies.

The following table exhibits the mean values resulting from such a combination. In the last three columns are given the intensities of the three kinds of light in terms of lamp-light, *all being supposed equal in the yellow.* The fifth and sixth columns are obtained by multiplying the second and third columns throughout by a proper factor:

Wave-length.	Moonlight.	Sunlight.	Lamp-light.	Moonlight.	Sunlight.
μ					
0.687	.032	.055	1.00	.41	.26
0.649	.041	.096	1.00	.52	.46
0.599	.066	.165	1.00	.84	.79
0.586	.079	.209	1.00	1.00	1.00
0.518	.190	.696	1.00	2.41	3.33
0.486	.370	1.092	1.00	4.69	5.22
0.470	.592	1.890	1.00	7.50	9.03
0.415	1.050	3.540	1.00	13.29	16.92

Plate 4 is a graphical reprsentation of this table. The intensity of lamp-light is represented by a straight line everywhere at the distance 1 from the axis of λ. The sunlight and moonlight curves intersect this line at the point $\lambda=0^{\mu}.586$. They rise rapidly towards the violet end, but the sunlight ordinates increase faster than the moonlight ones. These curves show that the proportion of violet in sunlight is much greater than in moonlight, although as a quantitative determination the observations are not entirely satisfactory. The principal cause of error is, as already mentioned, the deficiency of violet rays in the light from the comparison lamp. The errors of observation become more apparent on eliminating this intermediate term, and comparing directly the light from the sun with that from the moon. From the curve in the figure we obtain the first part of the following table, and by a graphical construction of this part we get the last two columns from a smooth curve. This curve, as given by the table, is concave towards the axis of λ. It is quite certain, however, that if the observations had been perfect and made under

similar circumstances it would be convex. It is evident that the sunlight in these measures is at a great disadvantage in respect to the moonlight, especially in the upper regions of the spectrum, since the violet light from the sun, which was observed at a much lower altitude, had been more powerfully absorbed by the atmosphere. This absorption was even greater than could be expected from the mere difference in altitude, for the sky at night was almost invariably better than in the daytime, and, moreover, the cloud of smoke, which always hangs over the city of Pittsburgh towards the south, gives an absorption for large zenith distances much greater than the mass of air traversed would produce alone.

If the observations had been made under precisely similar circumstances, the preponderance of violet in the solar spectrum would be far more pronounced.

Wave-length.	$\frac{\text{Sunlight.}}{\text{Moonlight.}}$	Adopted $\frac{\text{Sunlight.}}{\text{Moonlight.}}$	Adopted $\frac{\text{Moonlight.}}{\text{Sunlight.}}$
μ			
0.687	.65	.68	1.47
0.649	.88	.81	1.23
0.599	.95	.96	1.04
0.586	1.00	1.00	1.00
0.518	1.23	1.18	.85
0.486	1.35	1.26	.79
0.470	1.35	1.31	.76
0.415	1.28	1.43	.70

In addition to this table we give another, containing the results obtained by different observers, reduced by intepolation from smooth curves to the same points measured on in 1884. As some of these measurements were made under circumstances exactly opposite, as regards the relative heights of the sun and moon, to those we have described, we may expect from a combination of them all to obtain a result more nearly free from the effects of unequal absorption of the light from the two bodies by the atmosphere.

Relative intensities of sunlight and moonlight.

Wave-length.	Pickering.	Preliminary observations of 1883.	Observations of 1884.	Vogel.	Mean* by weights.	$\frac{\text{Moonlight.}}{\text{Sunlight.}}$
μ						
0.687	.48	....	.68	.90	.70	1.43
0.649	.64	....	.81	.92	.77	1.30
0.599	.89	.7	.96	.98	.92	1.08
0.586	1.0	1.0	1.00	1.00	1.00	1.60
0.518	2.2	6.5	1.18	1.26	1.68	.60
0.486	4.6	9.5	1.26	1.40	2.37	.42
0.470	6.3	13.	1.31	1.54	2.72	.37
0.415	13.	20.	1.43	2.10	4.22	.24

* The values in the sixth column have been used as ordinates for the curve. (Plate 5.)

In obtaining the column headed "Mean" the weight 5 has been given to each of the two preceding columns and the weight 1 to each of the others. The observations of Mr. PICKERING on the moon were made under unfavorable circumstances, and the light ratio in the violet depends upon a single series of three readings. Those made at this observatory in 1883 were for the purpose of experimenting on the best arrangement of apparatus, and not made with a view to obtain the best quantitative results, while the values given by Dr. Vogel and by the Allegheny observations of 1884 are the results of many and careful observations throughout the entire range of the spectrum. The weight which we have assigned to them would not therefore appear to be too great.

With the aid of this table we may make an effort to draw the lunar energy curve. Within the limits of our observations an increase in energy in a definite part of the spectrum is followed by a proportional increase in brilliancy,* so that the figures in the last column, which represent

* With intenser lights than we employ certain physiological phenomena affect this proportionality, which is here, however, sensibly exact.

the light intensity ratio of moonlight and sunlight, may also be taken as the ratio of the ordinates of the lunar and solar energy curves.

For the ordinates of the normal solar energy curve we may take the values given by the mean of all noon observations made with the spectro-bolometer at Allegheny during the spring of 1881. Then, multiplying each ordinate by the corresponding factor given in the last column of the preceding table, we obtain the ordinates of the lunar energy curve. The results are exhibited below in tabular form:

Wave-length.	Solar ordinates.	$\frac{\text{Lunar energy.}}{\text{Solar energy.}}$	Lunar ordinates.
μ			
0.687	575	1.43	822
0.649	604	1.30	785
0.599	624	1.08	674
0.586	622	1.00	622
0.518	590	.60	354
0.486	535	.42	225
0.470	490	.37	181
0.415	300	.24	72

If we wish the lunar energy curve to represent the distribution of the same amount of energy as the solar within the limits of the visible spectrum (say between $0^{\mu}.4$ and $0^{\mu}.7$), we must multiply each of the lunar ordinates by the fraction $\frac{329}{284}$, which is determined by plotting the curves and measuring the areas within the required limits. We obtain by this operation the following table, which is also graphically represented by the curves in Plate 6.

Wave-length.	Solar ordinates.	Lunar ordinates.
μ		
0.687	575	952
0.649	604	909
0.599	624	780
0.586	622	721
0.518	590	410
0.486	535	261
0.470	490	209
0.415	300	83

An inspection of these curves shows at once the effects of the selective absorption undergone by the solar rays at the moon's surface. The maximum ordinate of the lunar curve falls much lower down in the spectrum, and there is a corresponding reduction in the height of the curve over the violet end. The visible part of the normal spectrum forms, however, so small a portion of its entire length, that it would be unsafe to judge from the nature of the lunar curve obtained by optical means, as to its probable course at points very far below the limit of the visible red. Nevertheless, the evidence of these photometric measurements as to the selective reflection exercised by the moon's surface is, as far as it goes, decisive, and it is shown to be in such a direction as to cause a preponderance in the lunar spectrum of the rays of long wave-length, and hence to tend to cause a smaller percentage of lunar rays to be transmitted by glass than of solar, and this independently of any effect from heat reradiated by the lunar soil. There is, then, no doubt that the observed phenomenon of glass absorption already described is due in part to this cause, though in how large part we do not now determine.

ADOPTED HEAT-MEASURES WITH BOLOMETER AND GALVANOMETER.

The galvanometer is so important an accessory of the bolometer, that we will describe the arrangement we have used to make our own most effective.

The galvanometer employed is a Thomson differential astatic galvanometer, having a resistance of 20.35 ohms, and originally made by Elliott Brothers, with a short suspending fiber, a damp-

ing magnet sliding on a brass rod, and a system of five upper and five lower magnets connected by an aluminium rod with an aluminium vane, the time of a single vibration without damping magnet being 6.58 seconds.

In preparation for the extremely delicate final work on the moon, the following changes were made: (I have to express my great obligations to the kindness of Prof. Sir William Thomson and of Professor Rowland for valuable suggestions.) The most important of these improvements has been the replacing of the short fiber by one 33 centimeters in length (for the brass rod being substituted a hollow glass one, in the center of which is the fiber); and, second, the reconstruction of the needle.

In the new astatic system constructed at this observatory in November, 1884, the aluminum rod carrying the magnets was replaced by a hollow glass fiber. The aluminum vane, it occurred to me to replace by an insect's wing, and one was most advantageously made of dragon-fly's wings, (in which nature has supplied an admirably rigid and light construction). A minute platinum paddle at the bottom of the glass fiber, touching the surface of oil in an oil-cup, was supplied, and a new system of magnets. These are made by rolling soft sheet-steel, 0.076 millimeters thick and 5 millimeters wide and from 7 to 9.5 millimeters long, around a short straight piece of wire into minute cylinders, carbonizing them in fused ferrocyanide of potassium, and tempering them in mercury. The strength of one of the little magnets was found to be 874 Gaussian units, and of these there are in all twelve, six on each system.*

In forming the connections, it will be found advantageous to employ a battery of a considerable number of cells (*e. g.* twelve, of a gravity battery), and to reduce the current by interposing resistance. Under these circumstances, it might appear that there was no advantage in using the current from twelve cells over that of one, if the current were as strong in either case. Such, however, is not the fact; for the accidental fluctuations due to the minute casual changes which take place in the most constant cell are obviously equalized by the use of a current which is the mean of that from a considerable number of cells.* Pains are taken to wrap every connection and binding post in cotton, and a great number of minute precautions, which are not here detailed, have been observed.

The damping magnet is arranged so as to take any position between the bottom of the glass rod and a point 1.46 meters above it, a graduated vertical scale being provided above the galvanometer rod. The mirror of the instrument is a minute silver-on-glass concave reflector, of 1-meter radius of curvature. The transparent scale, which is on the west, at 1 meter distance, is a portion of a cylinder of 1-meter radius, and is graduated in millimeters from 0 to 500. Accordingly, when the needle points north and south, and the optical axis of the mirror east and west, the image is at 250, at the middle of the scale. This image is a circle of light about 3 centimeters in diameter, with a central vertical line (the shadow of a wire).† With these values, to carry the image wholly off the scale demands a rotation of the needle through only about 7 degrees. As a rule, this small maximum deviation, with the employment of a curved scale, renders reduction for arc unnecessary in such observations as these. The needle, when rendered as astatic as possible, performs a single vibration in about a minute; but in this condition the directive force is apt to vary from one day to another, and the time of vibration, as a rule, to grow more rapid until a shorter period is reached, at which it becomes relatively constant. For the purpose of forming an approximate estimate of the sensitiveness of the instrument, it may be stated that when making a single vibration in 10 seconds a deflection of one millimeter division on the scale is given by a current approximately equal to 0.0000000013 ampère.

East of the galvanometer, and nearly in the prolongation of the optical axis of the upper mirror, are two small bar magnets, on an independent stand, a minute movement of either of which serves to bring the image on to any point of the scale when necessary without altering the resistance in the resistance box.

* The device of the hollow magnets is due to Mr. F. W. Very, of this Observatory, at whose suggestion also the number of battery cells was increased with great advantage. The actual construction and astaticising of the needles also has been chiefly due to Mr. Very's patience and skill.

† The employment of a telescope and a flat mirror, reflecting the inverted scale, is in some respects preferable to this arrangement, which is continued in use, however, at present from its greater facility of adjustment.

The adjustments are commonly made so that heat falling upon the bolometer causes a deflection of the image to the south, thus increasing the reading on the scale, whose zero is at the northern end. It may be added, in further indication of the sensitiveness of the instrument, that on bolometer No. 1 (whose resistance is 80.5 ohms) by Matthiessen's table, the change of temperature, corresponding to a change of resistance of 0.0001 ohm, is 0°.00032 C. Accordingly, when the needle is in such a condition of sensitiveness that it executes a single vibration in 10 seconds, and if we employ a current of 0.1 ampère, a change of one division on the scale corresponds to a change of temperature in the bolometer strips of 0°.000016 C. This result is to be understood as merely approximate, and as indicating nearly the limit of sensitiveness attained in actual work at present. It need hardly be added that greater nominal sensitiveness can be obtained to almost any extent by increasing the time of swing; but the gain is apt to be only nominal, for we are to consider that, other things being equal, the efficiency of the instrument increases as the probable error diminishes, where this probable error is expressed as a fraction of the deviation in question. In fact, as the concentrated moonbeam drives the image off the scale altogether in the above condition of sensitiveness, it is necessary to employ the damping magnet, not to increase, but to diminish, the time of vibration, so that the image may remain on the scale. Under these latter conditions the probable error of a single observation is very small, probably not exceeding 2 per cent.*

DESCRIPTION OF BOLOMETERS EMPLOYED.

Bolometer No. 1, which has been chiefly used in measurements of total lunar radiation when concentrated by the concave mirror, and for comparative observations with the Leslie cube, has a square central aperture, 8.3 millimeters on a side, through which the blackened strips of the central or exposed arm may be seen, presenting to the incoming rays an area of 49 square millimeters, and composed of 23 thin strips of blackened platinum, each about 0.001 millimeter thick, in two tiers, the rear ones covering the apertures left between the front ones. The other, or protected arm, is made up of 24 strips, an extra protected strip being introduced in the circuit of the exposed arm, to balance the resistance, which is 80.5 ohms for either arm. The case is a cylinder of ebonite, projecting so far beyond the strips as to cut them off from all radiations, except those from the subject of experiment.

Bolometer No. 13 is composed of 8 side strips and 9 central ones, each 0.25 millimeter wide, the latter forming a band 2.3 millimeters wide and 10.3 millimeters high, with which measures in the lunar spectrum have chiefly been made. Each arm resists 38.4 ohms.

Our direct observations on the lunar heat may be grouped under six divisions. (1) Quantitative measurements of lunar heat as compared with solar; (2) comparisons of the moon's heat with that from a terrestrial source; (3) the comparative transmissibility of our atmosphere for lunar and solar heat; (4) comparative transmissibility of glass for lunar and solar heat; (5) heat observations during a lunar eclipse; (6) the formation of a lunar-heat spectrum.

CLASS I.—QUANTITATIVE MEASUREMENTS OF LUNAR HEAT AS COMPARED WITH SOLAR.

Let us expose the bolometer to the lunar radiation, either direct or concentrated, and note the resultant galvanometer deflection, and repeat the experiment the next day with the solar radiation, diminished in a known ratio. If the moon be full and at an equal altitude with the sun at the time of observation, we have the direct ratio of heat received from each at the earth's surface but it is to be remarked that we cannot confine these observations to the single night of full moon without giving inordinate time to the research (since they should be often repeated); while, if we take them at times much before or after the full, considerable errors may be introduced by our ignorance of the true law of the variation of the moon's heat with the phase. Where we have been obliged to use the latter class of observations, we have reduced them by Zöllner's law. It is to be observed, also, that it is not only more than doubtful whether the transmissibility of the atmosphere

* It appears in Lord Rosse's observations that the mean of a series of 10 gave a probable error of 19 per cent. with the thermopile and galvanometer then employed. Accordingly, if we do not consider constant errors, but only accidental ones, we find that, owing to the increased sensitiveness and steadiness of our apparatus, a single measurement with the present train is equivalent to several hundred of that employed by Lord Rosse.

is the same by night as by day, but that other circumstances add to the difficulty in forming exact conclusions.

The apparatus employed in the following observations for lunar rays consisted of the concave mirror and bolometer shown in Plate 1. These were used at night, while in the day the sun's rays passed through a narrow aperture and fell on the bolometer placed at a considerable distance in the divergent beam.

The following are the principal constants of the apparatus:

Let S=lunar apparent semi-diameter at the time of observation, obtained from the geocentric semi-diameter corrected for augmentation.

f=the focal length of concave mirror=73.4 centimeters.

A=the radius of lunar beam falling upon the concave mirror=10.2 centimeters. This radius is limited by that of the hole in the north wall by which the beam from the siderostat enters.

Let s be the semi-diameter of the lunar image in the focus of the concave mirror. Then

$$f \sin S = s, \text{ and } \frac{A^2}{s^2} = \text{concentration of lunar beam.}$$

The absorption by the silver of the mirror and the loss by non-perpendicular incidence of the rays on the bolometer strips are here neglected.

Again, let a be the semi-diameter of the aperture used for transmitting the solar rays, S′ the solar semi-diameter, and d the distance of the bolometer strips from the diaphragm. Then $\pi (d \sin S' + a)^2 = \pi l^2$, where l is the radius of the circle formed by the divergent solar beam at the distance of the bolometer strips, and, neglecting the effect of diffraction at the diaphragm, the diminution of the solar beam $= \frac{a^2}{l^2}$. The ratio $\frac{A^2}{s^2}$ to $\frac{a^2}{l^2}$ is that of sun heat to moon heat, or rather that from an element of the center of the sun's surface to the mean value of the heat from the full moon. Observations of this kind were made on the following evenings, November 29, December 2, and December 3, 1884.

As an example we give that of December 2, 1884:

Time.	Zenith distance.	Deflection.	Time.	Zenith distance.	Deflection.
h. m.	° ′		h. m.	° ′	
6 24	72 24	174	9 39	38 24	303
6 36		188	9 41		312
7 59	54 22	273	9 45		311
8 15		285	9 47		307
8 21	52 05	295	9 51		305
8 24		301	9 55		317
8 27		311	11 58	22 22	326
8 30		298	12 02		332
8 32		297	12 04		315
8 35		309	12 07		331
			12 09		313
			12 13		318

The sky during these observations was quite good and cloudless. A light haze having gathered shortly after midnight, observations were discontinued before the moon had quite reached the meridian.

On the following day (December 3) observations were made on the sun at noon. The state of the sky appeared to be about the same as during the preceding lunar observations, and the battery current employed was adjusted to the same strength.

The following observations were obtained:

Sun's hour angle.	Deflection.
min.	
−10	163
− 7	181
+ 7	170
+10	196
+15	174
	Mean . . 176

One hundred and seventy-six divisions was therefore taken as the deflection produced by the sun at apparent noon, when his zenith distance was 62° 43′.

On drawing a smooth curve through points given by the lunar observations in the first table, we find that the deflection produced by the moon at the same zenith-distance was 245 divisions. On the evening of December 2 the moon's geocentric semi diameter was 16′ 47″, and the semi-diameter at the time and place of observation was 16′ 55″. The focal length of the concave mirror being 73.4 centimeters and the diameter of the moonbeam 20.3 centimeters, the concentration of the moonlight was

$$\frac{(20.3)^2}{(73.4+\sin 16'\,55'')^2}=\frac{(20.3)^2}{(.722)^2}=790.5$$

The aperture through which the sunlight was admitted was 0.486 centimeter diameter, and the bolometer strips, when exposed, were distant from it 653.5 centimeters. The sun's semi-diameter at noon being 16′ 16″.5, the approximate diminution of solar light and heat was

$$\frac{(0.486)^2}{(653.5\times\sin 16'\,16''.5)^2}=\frac{(0.486)^2}{(6.674)^2}\ .00530$$

The moon was not quite full at the time of observation. We find by Zöllner's formula that if it had been, the deflection produced would have been $\frac{245}{.90}=272$ div. We have, then, for the observed ratio of radiation from the full moon to that of the sun, both bodies being at a zenith distance of 62° 43′,

$$\frac{272}{176}\times\frac{1}{790.5}\times\frac{530}{100,000}=\frac{1}{96509}$$

values which the reader is again reminded are presumably subject to large constant errors. The maximum total heat which we can by possibility receive from the moon, even in the absence of an absorbing atmosphere, as is shown elsewhere, is about 1–97000 of the solar heat. It is improbable that such a coincidence as that presented with the observed value just given is other than largely the result of chance, or rather of such constant errors tending in an unknown degree to increase the observed values.

CLASS 2.—COMPARISON OF THE HEAT FROM THE MOON WITH THAT FROM A LESLIE CUBE.

On December 3, 1884, the temperature of the room being 0° C., the bolometer was exposed to the radiation from a Leslie cube filled with boiling water, which was observed through the circular aperture of a screen subtending the same angle as the cone of rays from the concave mirror used in

lunar measurements. The following deflections were observed, the sensitiveness of the measuring apparatus being the same as during the lunar observations of the previous evening:

Temperature of Leslie cube.	Galvanometer deflection.
C.	Div.
95	408
92	400
89	384
86	370
83	369
81	353
73	297

From a smooth curve we adopt 435 as the presumable deflection, which would have been observed under these conditions at 100° C. The screen itself acquired a minute amount of heat during the experiment, but the correction for this is negligible.

The bolometer strips attain thermal equlibrium under ordinary circumstances within a fraction of a second, while on account of the slowness of change of the temperature of the case, we can assume its radiation (C) to be constant during the experiment. The temperature of the bolometer strips may always be taken to be proportional to the angular area of the part of the surface radiating to them, to its temperature, and to its emissive quality.

Thus the aperture of the moon bolometer occupies 0.00565 of the sphere. The temperature of the room December 3, 1884, was 0°.0 C. If the aperture had pointed to a surface at the absolute zero, having the same emissive power as its case, a fall of temperature of 0.00565 multiplied by 273° = 1°.542 would have been experienced. We assume that, within the limits of this experiment, the Newtonian law of radiation holds. If the pointing had been to a surface at 100° C. of the same emissive power, the temperature of the bolometer would have risen 0°.565. Now, we have seen that a Leslie cube at 100° C. would have produced a deflection of 435 divisions on the galvanometer. One division, therefore, indicated $\frac{0°.565}{435}=0°.0013$ change of temperature of the bolometer strips (the full sensitiveness of the galvanometer not being used). The deflection produced by the full moon on the previous evening, if reduced to zenith, would have been over 350 divisions, and the temperature of surrounding objects being 0° C. the effective radiation of the moon, if we suppose its emissive power the same as that of the case, was such as would correspond to a temperature of $\frac{350}{435}\times 100° = +80°.5$ C., or 80°.5 C. above the temperature of surrounding terrestrial objects, which happened to be zero Centigrade. This on the absolute scale gives $80°.5+273°=353°.5$; and if one-fourth of the lunar radiation is reflected sun heat the true average temperature of the moon at the full is $80°.5-\frac{1}{4}\times 353°.5 = -7°.9$ C. If we assume that one-half only is reflected sun heat, we have $-99°.3$ C., if that one-sixth is reflected, $+21°.6$ C.

A correction for atmospheric absorption which we have not applied would somewhat increase these values, but it is evident that not only the experimental conditions here do not favor accuracy but that the results, such as they are, are subject to a wide latitude of interpretation.

Class 3.—Transmission of lunar heat by the earth's atmosphere.

The remarks already made as to the difficulty of comparing observations at different altitudes but at different phases, when the law of change of heat with the phase is so imperfectly known, apply with peculiar force to this class of observations. Only a series of observations made exclusively on the full moon in favorable circumstances (and therefore occupying many years) could bring anything like satisfactory evidence. The reductions which we now give of the few values we possess lead to conclusions to which we cannot attach great weight.

The observations of December 2, 1884, are given below in tabular form, with the computa-

tions necessary for obtaining the coefficient of transmission by the atmosphere. This coefficient, as has been explained elsewhere*, is found by means of the formula

$$\log t = \frac{\log d_2 - \log d_1}{M_2\,\beta_2 - M_1\beta_1}$$

and from this may be found the original energy of the observed radiation before it entered the atmosphere,

$$\log E = \log d_1 - M_1\,\beta_1 \log t$$

although these formulæ are strictly applicable only to homogeneous rays, and hence give only approximate results. Each "deflection" is the mean of a number of observations made nearly at the same time.

Lunar heat observations of December 2, 1884.

[Height of barometer β—7.34 decimeters.]

Observation.	Time.	Hour angle.	Zenith distance.	M.	$M\beta$.	Deflection.	Deflection corrected for change of phase.
	h. m.	*h. m.*	°				
g	6 24	5 39	74 28	3.68	27.01	174	183
f	6 36	5 28	72 24	3.28	24.08	188	197
e	7 59	4 09	57 25	1.865	13.69	273	284
d	8 15	3 53	54 22	1.718	12.60	285	294
c	8 28	3 41	52 05	1.627	11.94	302	311
b	9 46	2 26	38 24	1.277	9.37	309	315
a	12 05	0 13	22 29	1.080	8.00	323	323

The comparison of observations made at great and small zenith distances is also exhibited in the form of a table.

Observations compared.	d_1	d_2	Log d_1	Log d_2	Log $\frac{d_2}{d_1}$	$-\text{Log}\left(\frac{\text{Log } d_2}{\text{Log } d_1}\right)$	Log $(M_2\beta_2 - M_1\beta_1)$	Log (Log t)	Log t	t	$\frac{-M_1\beta_1}{\text{Log } t.}$	Log E.	E.
a and *g*	323	183	2.5092	2.2625	−.2467	9.3921	1.2790	8.1131	—.0130	.971	.1040	2.6132	410
a and *f*	323	197	2.5092	2.2945	−.2147	9.3318	1.2062	8.1256	—.0134	.970	.1072	2.6164	413
a and *e*	323	284	2.5092	2.4533	−.0559	8.7474	0.7551	7.9923	—.0098	.978	.0784	2.5876	387
b and *g*	315	183	2.4983	2.2625	−.2358	9.3726	1.2465	8.1261	—.0134	.970	.1256	2.6239	421

The average value of t is 0.972, which is the fraction of the lunar radiation transmitted by a column of air capable of supporting 1 decimeter of mercury. The fraction of a vertical beam transmitted by the entire depth of the atmosphere would be $t^{7.6}$=.806.

The correction due to the change of the phase of the moon during the course of the night's observations is taken from a curve based on the formula of Zöllner.

CLASS 4.—COMPARATIVE TRANSMISSION OF GLASS FOR LUNAR AND SOLAR HEAT.

The pieces of glass used were the same as those employed in the preliminary experiments. They were *A*, *B*, and the "large window pane." A series of observations made by moving the siderostat mirror so as to expose alternately to the adjacent sky and to the moon gave, as has already been said, systematically different results from those obtained by the interposition of a screen and other modes of observation. For reasons already given, the values found by alternate exposure to the moon and sky are preferred. We give as an example the observations of December 3, 1884, on the sun, and of November 26, 1884, on the moon, the general disposition of the apparatus employed being that indicated in plate 1, and the glass, thoroughly dried and cleaned, being in ev-

*American Journal of Science, Vol. 125, page 176.

ery case allowed to communicate its own temperature to the bolometer before being exposed to the lunar or solar rays.

Transmission of solar radiation by glass.

[December 3, 1884. Sun's zenith distance at apparent noon 62° 43'. Sunbeam diminished in intensity to about 0.00530. The transmission is obtained by dividing the deflection, when the rays pass through glass, by the mean of the adjacent deflections in the direct solar beam.]

Mean time.	Deflection in direct solar beam.	Deflection with glass interposed.	Transmission of solar radiation by glass
h. m.			
11 40	163	....	....
11 42	....	125	0.727
11 43	181	....	....
11 57	170	....	....
11 59	....	133	0.727
12 00	196	....	....
12 05	174	....	....
12 07	....	130	0.791
12 08	155	....	....
12 10	....	123	0.794
12 12	155	....	....
12 13	....	112	0.725
12 15	154	....	....
12 16	....	119	0.761
12 17	159	....	....
12 19	....	118	0.735
12 20	162	....	....
		Mean.	0.751

Transmission of lunar radiation by glass.

[November 26, 1884. A good sky. External temperature—5°.0 C. Barometer, $\beta = 7.26$ decimeters.]

Mean time.	Moon's distance from meridian.	Moon's zenith distance.	Air mass. $M\beta$.	Deflection in direct lunar beam.	Deflection through glass. Glass outside.	Deflection through glass. Glass inside.	Transmission.
h. m.	*h. m.*						
5 56	0 48			240			
6 00	0 44	44½	10.2		22		.090
6 07	0 38					35	.139
6 11	0 34			257			
6 19	0 26				29		.114
6 24	0 21					30	.119
6 31	0 14			251			
6 53	0 06	43½	10.0	260			
7 00	0 13				26		.102
7 04	0 17				(2 layers=18)		
7 07	0 20					28	.112
7 14	0 28	44	10.1	247			
7 19	0 33				34		.140
7 25	0 39					30	.127
7 31	0 42	44½	10.2	231			
9 12	2 20	53	12.1	227			
9 17	2 25	54	12.3		29		.135
9 21	2 29	54½	12.5			26	.126
9 26	2 33	55	12.6	196			
9 33	2 40	56	12.9		26		.134
9 36	2 43	56½	13.1			25	.130
9 40	2 47	57	13.1	192			

Lunar radiation: Mean transmission (glass outside) .... .119
Mean transmission (glass inside) .... .126

Mean of all observations .... .122

There is very little difference between the results for transmission with glass inside and outside the building, and this little may be entirely accounted for by the diffusion of the rays by the

irregular surfaces of the glass pane, which produces larger loss when the latter is outside at a distance from the instrument.

The solar energy which falls on the moon may be divided into two portions: a, that which is reflected or diffused; b, that which is absorbed by the lunar soil and reradiated. We may form some rude *à priori* notion of the relative value of these from the following considerations: Were the full moon a perfectly diffusive body and reflecting according to the law established by Zöllner's experiments, it should behave nearly as a flat disc would do, and return to us such a portion of the sun's energy as the angular area of its disc bears to that of the hemisphere $\frac{1}{97300}$, $(\frac{1}{n})$. Hence we may take this fraction to express the ratio of total lunar radiation—*i.e.*,$(a+b)$—in terms of solar radiation. The ratio of lunar to solar luminous radiation is here taken to be (roughly) $\frac{1}{400000}$—but the ratio of lunar nonluminous to solar nonluminous radiation, owing to the selective absorption of the lunar surface, is probably indefinitely greater. This latter ratio is unknown, but the larger it is the smaller is the portion which we must assign to radiated heat. If, for instance, the perfectly diffusive moon sends us $\frac{1}{n}$ of the total solar radiation, and the ratio of lunar to solar radiation within the limits of the solar spectrum be $\frac{1}{m}$, $\frac{n}{m}$ is the proportion which is diffused or reflected to us (a), and $1-\frac{n}{m}$ is that which is absorbed and radiated (b). Now, n is a little less than 100,000, and m varies with the degree of selective reflection in the lunar surface. If m be 600,000, one-sixth of the lunar radiation is reflected or diffused solar energy, and five-sixths absorbed and radiated from the soil. If m be 300,000, one-third of the energy is reflected, &c., and somewhere between these two values it seems probable that the ratio sought will lie. The heat sent earthward by the radiation from the lunar soil is almost certainly greater than that reflected or diffused; but our atmosphere is, according to what we have been hitherto accustomed to think, comparatively opaque to the first class of heat (that radiated from the soil) and comparatively translucent or diathermanous to the second, so that there seems an *a priori* probability that the true ratio between a and b, as it would present itself to an observer outside our atmosphere, will be altered by its absorption, and that actually observed at sea level be something different. It seems certain, at any rate, that the radiation of the lunar soil must be of a quality to which glass is nearly opaque, since the glass which we have employed in our own experiments is nearly opaque to the radiation from a source at 100° C

CLASS 5.*—HEAT OBSERVATIONS DURING A LUNAR ECLIPSE.

The only lunar eclipse observed at Allegheny was that of October 4, 1884. The eclipsed moon rose behind clouds, and the first observation, obtained when the penumbra was already passing off, was made while the moon was still partly obscured by haze. Under these circumstances little interest attaches to the observation, which need not be cited in detail. The inference from it, so far as any could be drawn, was that about the same amount of heat was received as was to have been expected had there been no previous eclipse.

REVIEW.

Let us now review our sources of information and weigh the imperfect and sometimes contradictory results each has brought us.

(1) *Direct measurement of lunar heat as compared with solar.*—Our direct comparison indicates that we receive nearly the whole proportion of solar energy from the full moon that we should expect to get from a diffusive disk of the same angular aperture. This heat must in reality be partly diffused and partly radiated, and we do not know (from the present observations) in what proportions these two kinds enter. So far as the observation itself is reliable, we may, however, infer that our atmosphere is permeable to most of the lunar heat of either kind, but the method is unfortunately subject to such large sources of constant error, that we cannot derive great confidence from the apparent agreement of different observations or even of different observers. It may be said, however, to create a certain presumption that the earth's atmosphere is diathermanous to heat of lower wave-length than has been heretofore supposed, *and of lower wave-length than appears to reach us from the sun.*

* Class 6, see *infra*.

(2) *Comparison of moon's heat with that of Leslie cube.*—If we may draw any inference from this class of observations it is that the sun-lit surface of the moon is not far from the freezing temperature, but not so far below as we might expect to find that of an absolutely airless planet.

(3) *Transmission of lunar heat by the earth's atmosphere.*—Our observations indicate a not materially greater coefficient of transmission for lunar heat than for solar; and though their limited number and the uncertainty of the correction for change of heat with phase render more certainty as to the fact desirable, we may (accepting them as probable) reason thus.

Previous observations both at Allegheny and Mount Whitney have shown that the solar rays are transmitted with greater and greater facility (except for cold bands) as the wave-length increases up to the point (near $\lambda = 3^{\mu}$) where they suddenly disappear altogether. This shows either that (1) the solar heat, which according to the customary assumption exists to an unlimited wave-length before absorption, has here been cut off by a suddenly absorbent action, like that of a cold band extending indefinitely below 3^{μ}, or (2) that, either through a precedent absorption of such rays in the sun's own atmosphere or their non-existence, no solar rays below 3^{μ} present themselves to our atmosphere for admission.

The first view is that which I have treated as most in accordance with received opinion. It is not, however, the only one, since the second is not to be absolutely rejected, considering our experimental ignorance of the laws of radiation from gaseous bodies for great wave-lengths. Of these two hypotheses we see that, according to the first, our atmosphere is quite opaque to all heat below 3^{μ}, and the writer's (unpublished) experiments show that heat above this point must come almost wholly from a source much above 100° C. In this view, then (unless we agree that the radiations from the lunar soil correspond to a source much above 100° C.), we conclude that sensibly none of them pass our atmosphere, but that what we receive is diffused and reflected heat coming within the range of the known solar energy spectrum, and transmitted with nearly the same facility as solar heat, or if with a little greater, because lowered in wave-length by selective reflection at the lunar surface, not by absorption and reradiation from the lunar soil.

In the second view, for anything we have absolutely known to the contrary, our atmosphere may be permeable to radiations of any wave-length below 3^{μ}, and we could draw no certain inference, even if the lunar radiation were more distinctly different in transmissibility than it is.

As a matter of fact, with the actually limited difference in the character of its transmissibility, a difference which, as so far determined, is of the same order as that of the error of observation, we have no ground then from this present class of observation (*i. e.*, class 3) for any absolute conclusion one way or the other. But we repeat it seems to be a probable inference from our whole work that the earth's atmosphere is more diathermanous to heat of extremely low refrangibility than has heretofore been supposed.

(4) *Comparative transmission of glass for lunar and solar heat.*—The evidence here, which at first seems to so directly support the view of a sensible radiation from the surface of the moon, proves on examination to be subject to other interpretation, for the observed effect is almost certainly due in part to a degradation of wave-length by selective reflection from the lunar soil.

We can draw no absolute conclusion, then, from this evidence at first in appearance so promising, though we may say that it certainly indicates an increased probability for the view that radiations from the lunar soil may be transmissible by our atmosphere.

(5) *Observations during a lunar eclipse.*—If our own observations in this respect are imperfect, those of Lord Rosse before cited are on the other hand clear. They appear to bear but one interpretation, that all heat from the moon disappears immediately that it passes into the earth's shadow, and there is no evidence of any being retained, for any sensible time, more than if it were reflected.

It is so difficult to conceive that while the moon has been storing heat during many days of sunshine, it can part with it instantly, so that the temperature of the whole earthward surface of the planet disappears in an inappreciable interval, that most will see in this observation an argument against the existence of any such heat sensible to us at any time whatever.

(6) *Formation of a lunar heat spectrum.*—The observations made here with the lunar heat spectrum are as yet incomplete. With improving experience and apparatus, we hope to make others which shall give information of a character no other means can furnish. (See note, *infra*.)

CONCLUSION.

While we have found abundant evidence of heat from the moon, every method we have tried, or that has been tried by others for determining the character of this heat appears to us inconclusive; and, without questioning that the moon radiates heat earthward from its soil, we have not yet found any experimental means of discriminating with such certainty between this and reflected heat that it is not open to misinterpretation. Whether we do so or not in the future will probably depend on our ability to measure by some process which will inform us directly of the wave-lengths of the heat observed.

Note added February, 1885.—Since the above paragraph was written, we have succeeded in obtaining measures with rock-salt prisms and lenses in a lunar *heat spectrum*. These difficult measures must be repeated at many lunations before complete results can be obtained; but, considering their importance to the present subject, we think it best to state now in general terms, and with the reserve due to the necessity of future experiment, that they indicate two maxima in the heat curve, one corresponding within the limits of errors of observation to the solar curve maximum, the second indefinitely lower down in the spectrum, corresponding to a greater amount of heat at a lower temperature. Exactly what temperature this latter corresponds to, we have no present means of knowing. We have succeeded, however, in forming a measurable heat spectrum from the surface of a Leslie cube containing boiling water, and the maximum ordinate in the lunar heat curve appears to be below the maximum ordinate in the hot water curve. The inference from this is, of course, that the temperature of the lunar soil is, at any rate, below that of boiling water and in an indefinite degree.

We cannot close this note without calling attention to the remarkable fact that we here seem to have radiations from the moon of lower wave-length than from the sun, which implies an apparent contradiction to the almost universally accepted belief that the sun's emanations, like those from any heated solid body, include all low wave-lengths representing temperatures inferior to those certainly emitted.

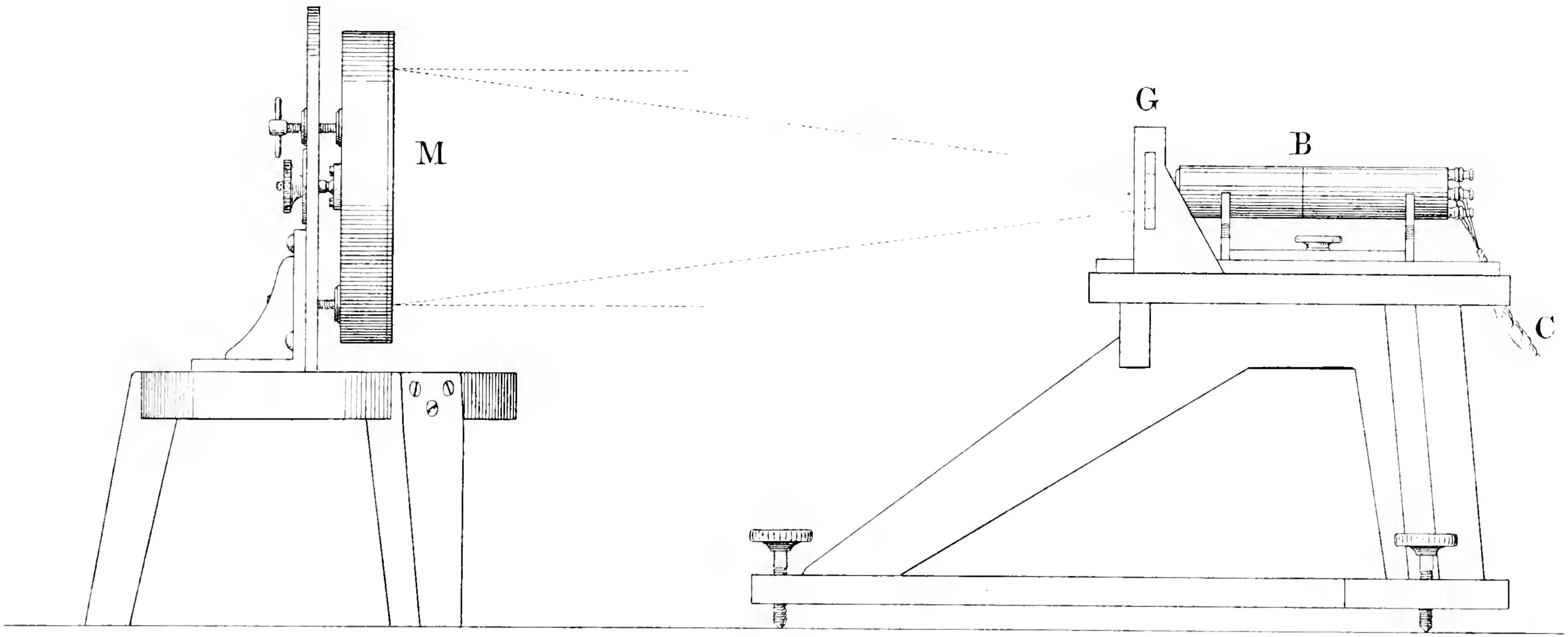


Plate 1.—Lunar Heat Apparatus.

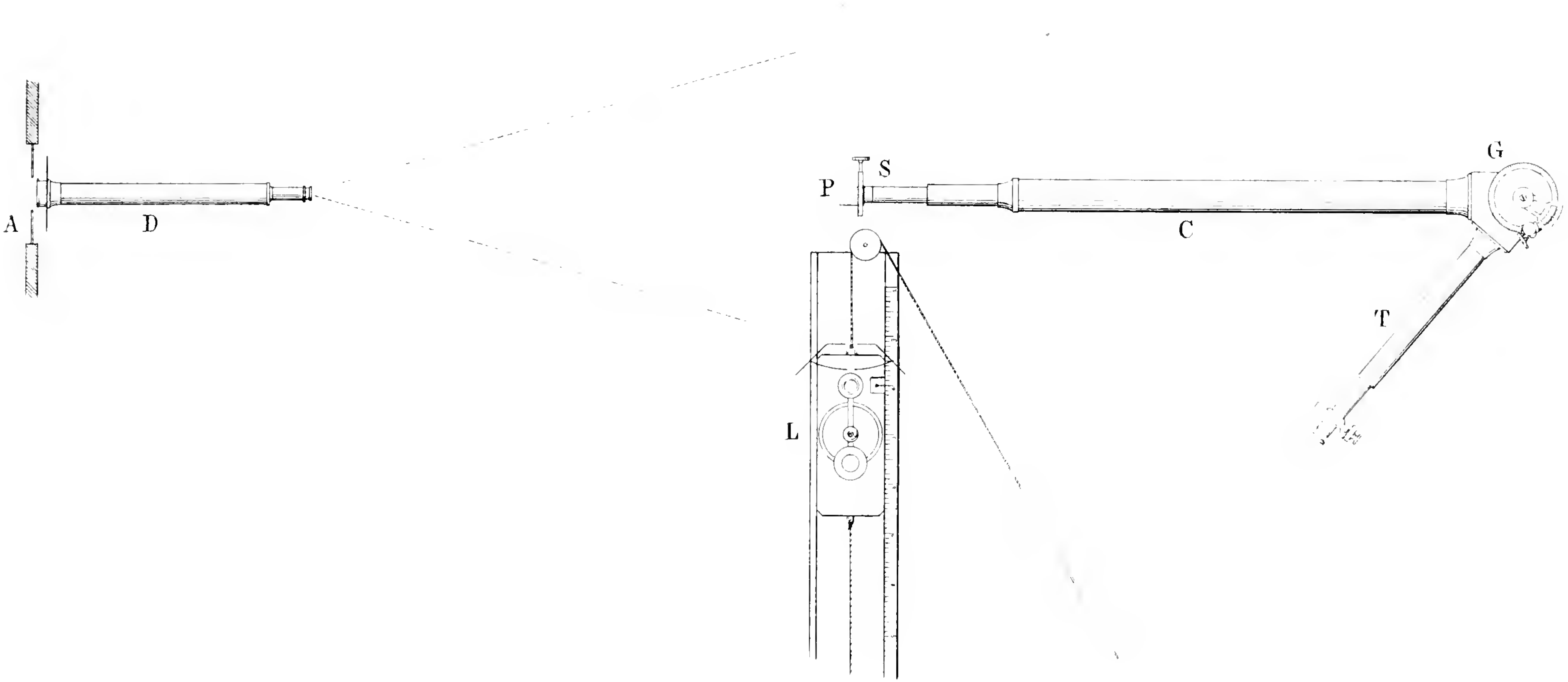


Plate 2.—Spectro-photometer.

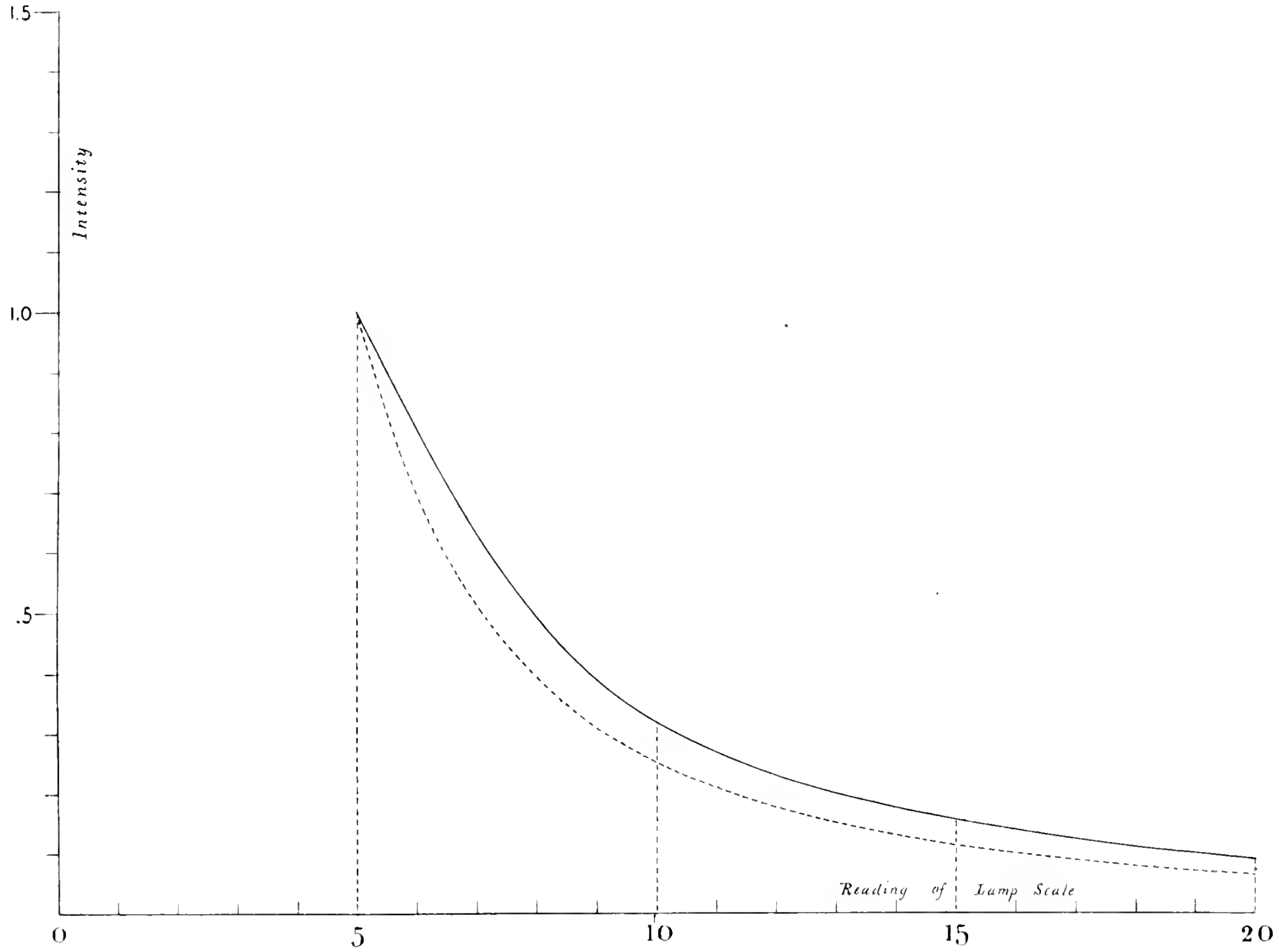


PLATE 3.—LIGHT-INTENSITY OF PHOTOMETER LAMP $= f$ (SCALE READING).

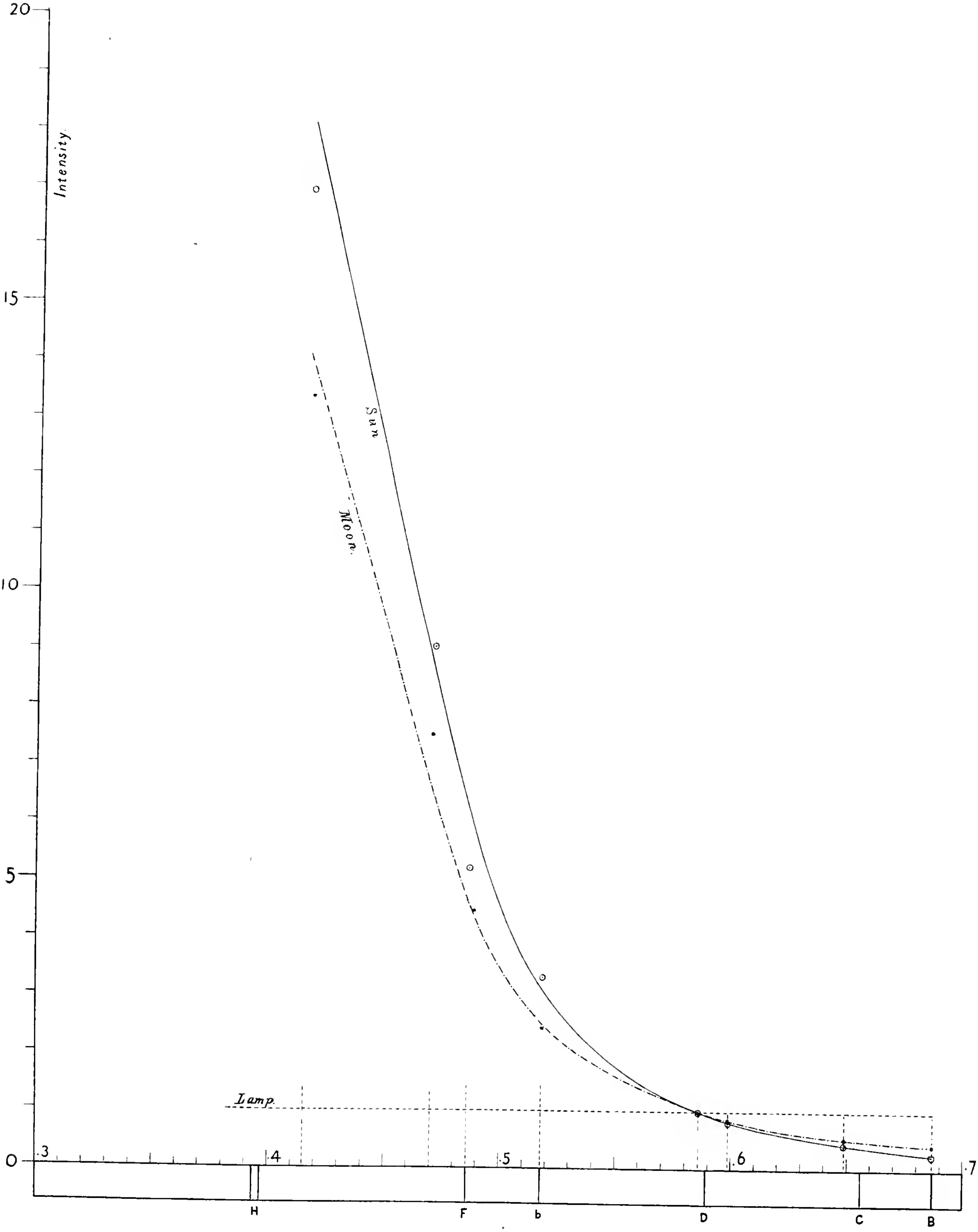


PLATE 4.—RELATIVE INTENSITIES OF SUNLIGHT, MOONLIGHT, AND LAMPLIGHT.

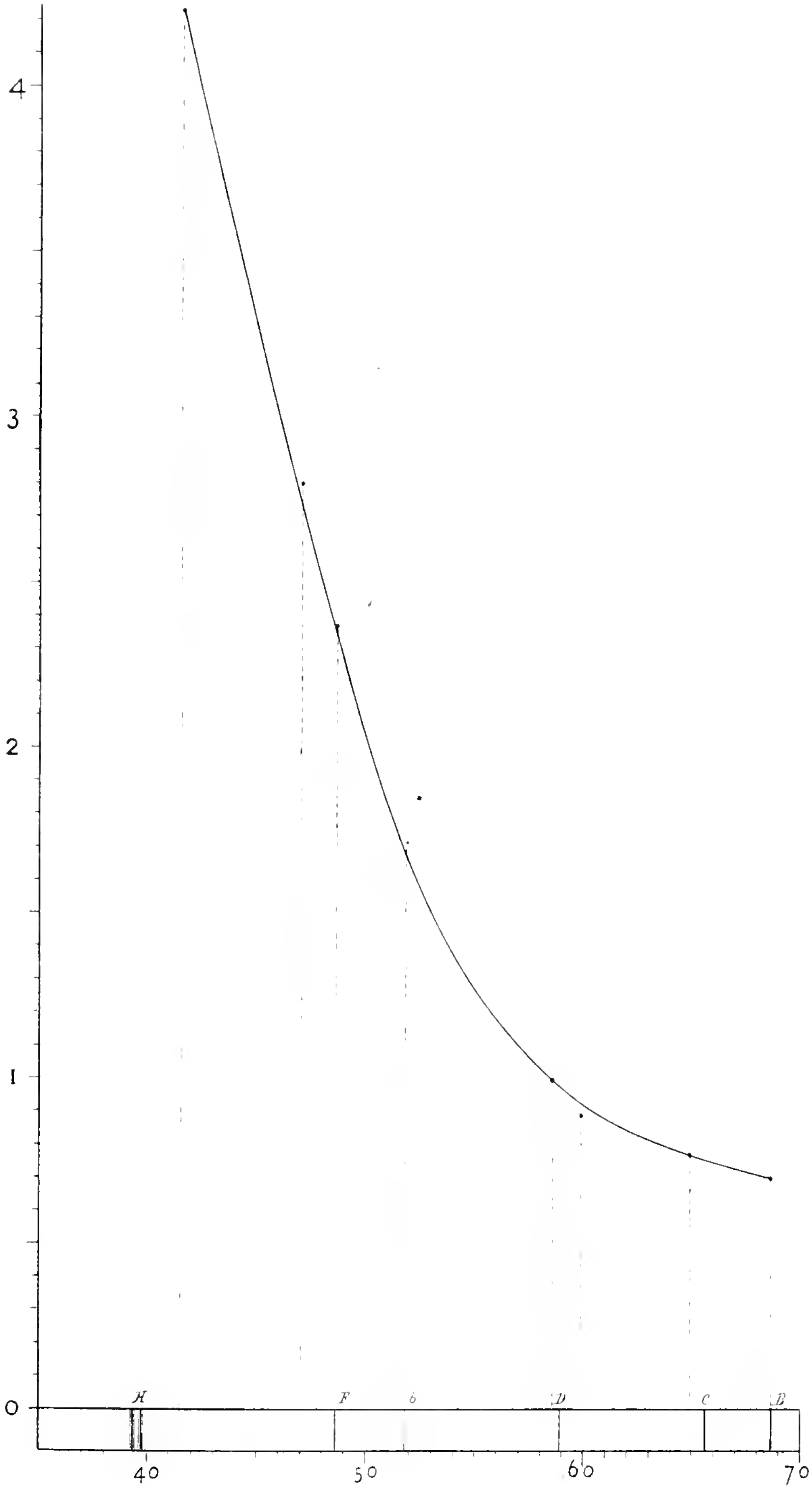


Plate 5.—Curve showing the Ratio of Sunlight to Moonlight in Different Parts of the Spectrum.

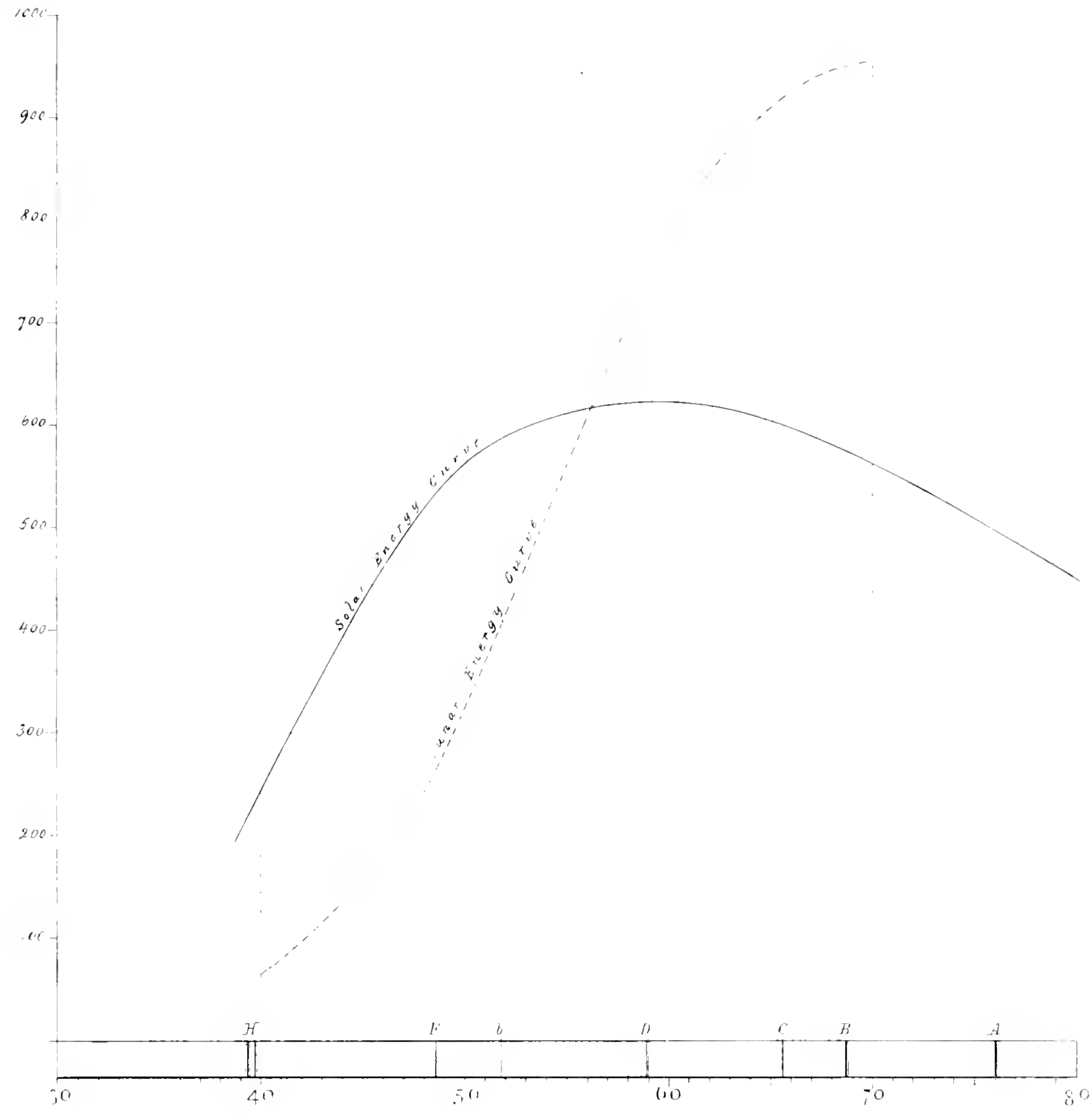


PLATE 6.—SOLAR AND LUNAR ENERGY CURVES.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

THIRD MEMOIR.

ON A METHOD OF PRECISELY MEASURING THE VIBRATORY PERIODS OF TUNING-FORKS, ETC.

ON A METHOD OF PRECISELY MEASURING THE VIBRATORY PERIODS OF TUNING-FORKS, AND THE DETERMINATION OF THE LAWS OF THE VIBRATIONS OF FORKS; WITH SPECIAL REFERENCE OF THESE FACTS AND LAWS TO THE ACTION OF A SIMPLE CHRONOSCOPE.

By Alfred M. Mayer.

This research was carried on with funds from the Bache endowment to the National Academy of Sciences. Its object was to arrive at a method of precisely measuring the vibratory periods of tuning-forks, and to determine the laws of the vibrations of forks, with the special reference of these facts and laws to the uses of the tuning-fork as a chronoscope in measuring small intervals of time.

The method devised is to make a clock, at each second, flash a spark of induced electricity on the trace made by a style attached to the prong of the vibrating fork *F*. *P*, Fig. 1, is the pendulum armed with a triangular piece of platinum foil, which, at each second, cuts through a globule of mercury contained in a small iron cup, *M*. This cup is so made that the globule can be regulated as to size and height by means of a screw-collar. Fresh mercury was placed in the cup at each experiment. The tuning-fork *F* is screwed into a board, *H*, which is hinged at *h*. This board rests against a screw-stop, *R*. *C* is a cylinder of brass, rotating on an axle, on one end of which is cut a screw, which runs in a nut at *T*. (See upper figure of Fig. 1.) The end of a prong of the fork is armed with a small triangle of thin elastic copper foil, about $\frac{1}{50}$ millimeter thick, and weighing only one milligram. The surface of the prong is well washed with ether, and then the foil is cemented to it with shellac. The point of this style just touches the camphor-smoked surface of paper, which tightly and smoothly envelops the cylinder *C*. The primary coil of an inductorium, *I*, and the clock (through *P*, and the globule of mercury, *M*) are placed in the circuit of a voltaic cell, *B*. In the secondary circuit of the inductorium is the fork *F* and the cylinder *C*, the thickness of the paper on the latter separating the point of the style on the fork from the surface of the brass cylinder. The fork is thrown upward, around the hinge *h*, vibrated by drawing a bow across a prong; then depressed till the board *H* comes against the stop *R*. The cylinder is rotated, and the trace of the fork is made on the paper, as shown in upper figure of Fig. 1. At each second, when the platinum-tipped pendulum *leaves* the globule of mercury, a spark flashes from the point of the style and makes *a single minute and circular* white spot on the blackened paper. This spot must be bisected by the trace of the fork. The center of the spot is generally marked by a minute perforation.

To obtain the results just described, it is necessary to fulfill certain conditions in the experiment, which, if neglected by experimenters, they would hastily regard the method as inaccurate. These conditions are as follows: (1) The globule of mercury should be small and *rigid;* that is, it should not vibrate when the platinum tip cuts through it. This condition is attained by screwing up the collar on the small cup *M* till only a small portion of the mercury is above the upper face

of the cup. This adjustment has to be made with great care. The spark which at each second passes between the platinum tip and the mercury rapidly oxidizes the latter, and the mercury must be renewed at each experiment. (2) The paper on the cylinder must be smooth, thin, but not glazed. It required many days of experimenting before I succeeded in getting a paper which gave the results I sought. The best paper is a very thin printing paper with a smooth, unglazed surface. (3) The style on the fork must be very light and elastic. The best for this purpose is made out of thin hard-rolled copper or aluminium. (4) The spark given by the inductorium must be of the character already described. If the discharge of the inductorium be not composed of *a single spark*, and its impress on the paper a minute circular spot *bisected by the trace of the fork*, it will be useless to expect accurate results from this method.

To reach these conditions cost much time, and it may be interesting to describe some of the variations in the character of the discharge of an inductorium when excited by various strengths of current, and when condensers of various areas are or are not in the secondary circuit.

The flash of an inductorium appears composed of a single discharge; but only in certain conditions is it really composed of a single spark. If the discharge be obtained through the style on the fork with a current traversing the coil of a strength approaching that used in the usual electrical experiments with it, several flexures of the trace of the fork will be obliterated by the discharge deflagrating the carbon on the paper. This effect is produced by a multiplicity of discharges, following each other with such rapidity, and of such strength, as to denude the paper of carbon, to some extent, on either side of the trace. The breadth of carbon removed and the fuzzy character of the contour of these traces give them the appearance of caterpillars.

To obtain an analysis of these complex actions I devised the following method of experimenting, shown in Fig. 2: A revolving brass cylinder, similar to the one used in our apparatus just described, was covered with thin printing paper, and the latter was well blackened by rotating the cylinder over burning camphor. The paper was then removed from the cylinder and cut into disks of about 15 centimeters in diameter. When one of these disks is revolved about its center with a velocity of about 20 times per second, it is rendered very flat by centrifugal action. It can then be brought between points or balls, even when the latter are separated by no more than .75 millimeter. When in this position the discharge between the points or balls perforates the disk and leaves a permanent record of its character, of the duration of the whole discharge, and of the intervals separating its constituent flashes and sparks. To obtain the time of rotation I presented momentarily to the rotating disk a delicate point attached to the prong of a vibrating fork of known period of vibration. The axis of the sinuous trace thus made by the fork is traced by a needle point applied to the rotating paper disk. Drawing radii through symmetrical intersections of this axis on the sinuous line, we divide the disk off into known fractions of time. The disk is now removed from the rotating apparatus and the carbon is fixed by floating the disk for a moment on dilute spirit varnish. When the disk is dry it is centered on a divided circle provided with a low-power micrometer microscope, and the duration of the whole discharge and the intervals of time separating its components can be determined to the $\frac{1}{25000}$ of a second. I here give three typical experiments with this method, which will show the characteristics of the discharge of an inductorium:

1. Discharge of a large inductorium (striking distance, 45 centimeters between brass points) between platinum points 1 millimeter apart. No jar or condenser in secondary circuit.

The platinum electrodes were neatly rounded and formed on wire $\frac{6}{10}$ millimeter in diameter. After the discharge through the rotating disk nothing was visible on it except a short arc formed of minute, thickly-set white dots; but on holding the disk between the eye and the light, it was found to be perforated with 33 clean, round holes, with the carbon undisturbed around their edges. The portion of the discharge which makes these holes lasts $\frac{1}{23}$ second, and the holes are separated by intervals which gradually decrease in size toward the end of the discharge, so that the last spark-holes are separated about one-half of the distance separating the holes made at the beginning of the discharge. The average interval between the spark-holes is $\frac{1}{759}$ second. After this

portion of the discharge has passed there is a period of quiescence lasting about $\frac{1}{1500}$ second; then follows a shower of minute sparks, which forms the short dotted line already referred to. This spark-shower lasts $\frac{1}{330}$ of a second, and is formed of 30 sparks; hence the average interval separating these sparks is $\frac{1}{9900}$ second. The intervals separating these sparks are, however, not uniform, but are smaller in the middle of the spark-shower than at the beginning or at the end of this phenomenon. This spark-shower, indeed, is a miniature of the phenomenon obtained when a Leyden jar is placed in the secondary circuit of the coil, and which will be described in the following experiments. The above determinations of intervals of time in the discharge are the mean of measures on six disks.

2. Discharge of large inductorium between platinum points one millimeter apart, with a Leyden jar of 242 square centimeters surface in the secondary circuit of the coil.

After this discharge through the disk a very remarkable appearance is presented. The discharge in its path around the rotating disk dissipates little circles of carbon. There are 91 of these circles, each perforated by 4, 3, 2, or 1 holes. I have to frame a new nomenclature to describe this complex phenomenon. I call the whole act of discharge of the coil, *the discharge*. Those separate actions which form the little circles by the dissipation of the carbon we will call *flashes*, and the perforations of these circles we call *spark-holes*. The discharge in the above experiment lasts $\frac{1}{24}$ second. The flashes at the beginning of the discharge are separated by intervals averaging $\frac{1}{555}$ second up to the tenth flash; after this the intervals of the flash rapidly close up, so that during the fourth fifth of the discharge they follow at each $\frac{1}{5550}$ of a second. During the last fifth of the discharge the intervals between the flashes gradually increase, and the last flash is separated from its predecessor by $\frac{1}{1000}$ of a second.

The appearance of the carbon-covered disk, after one of the discharges just described has passed through it, is given in Fig. 3.

On diminishing the current in the primary coil of the inductorium I found that the number of flashes in the discharge diminished, so that at last I obtained a discharge which consisted of but one flash perforated by one minute spark-hole. Also, if the current remain the same and a portion of the secondary circuit be divided, and gradually separated more and more, the number of flashes in the discharge will be diminished and the whole energy of the discharge concentrated in time. But no rule can be given for any special coil to obtain from it such a discharge as is alone useful in the work on the forks, and the current must be gradually varied by resistances in the primary circuit of the inductorium, and the area of the condenser in the secondary circuit, till the conditions for any special coil are obtained which cause it to give a spark which makes a minute circular and well-defined mark directly in the trace of the style of the fork. In the inductorium used there is 150 feet of wire in the primary circuit and eight miles in the secondary. The condenser in the secondary circuit was formed of tin-foil separated by panes of glass, and had an area of 50 square inches.

STUDY OF THE EFFECT OF VARYING AMPLITUDES OF VIBRATION OF THE FORK ON ITS VIBRATORY PERIOD; AND ON THE EFFECTS OF VARYING PRESSURES OF THE STYLE ON THE PAPER-COVERED CYLINDER.

The experiments on this fork of Kœnig's were made not so much for the determination of its vibratory period at a given temperature, as to discover any effect on the vibratory period caused by difference of amplitude of vibration, and by varying pressures of the tracing style on the smoked paper. This series of measures is given as an average example of series of similar sheets on which we have made measures. It will be observed that the vibration-numbers opposite the successive seconds, given in the first column, are alternately small and large. This is due to the fact that the center of the globule of mercury is not exactly on the vertical of the pendulum, but by taking

the mean of two successive seconds we have the mean number of vibrations for those seconds. These means are given in column 3.

TABLE I.

(1)	(2)	(3)
1	255.00	255.95
2	256.90	
3	255.05	255.97
4	256.90	
5	254.90	255.90
6	256.90	
7	254.70	255.92
8	257.15	
9	254.95	256.02
10	257.10	
11	254.90	256.00
12	257.10	

(1) The mean of 1st and 2d seconds = 255.95. Amplitude of vibration of 1st second = 2.03 millimeters.
The mean of 11th and 12th seconds = 256.00. Amplitude of vibration of 12th second = .63 millimeter.

From this observation one might conclude that the number of vibrations increased with a diminished amplitude, but the following observations show that this is not a just conclusion:

(2) Mean of 1st and 2d seconds=255.97. Amplitude of vibration of 1st second=1.19 millimeters.
Mean of 7th and 8th seconds=255.97. Amplitude of vibration of 8th second= .59 millimeter.
(3) Mean of 1st and 2d seconds=256.05. Amplitude of vibration of 1st second=2.39 millimeters.
Mean of 11th and 12th seconds=256.00. Amplitude of vibration of 12th second= .61 millimeter.
(4) Mean of 1st and 2d seconds=256.17. Amplitude of vibration of 1st second=2.07 millimeters.
Mean of 9th and 10th seconds=256.20. Amplitude of vibration of 10th second= .78 millimeter.

From the above measures we conclude that differences of amplitude of vibration in a fork, arranged as in the experiments, has no appreciable effect on its vibratory period.

Many measures were made on records obtained with varying pressures of the tracing style against the smoked paper; but the slight variations of those pressures which could be obtained within the range of elasticity of the delicate style used gave no differences in the number of vibrations from which we could detect any influence of varying pressures of the style.

EFFECT OF TEMPERATURE ON THE VIBRATORY PERIOD OF FORKS.

To determine the effect of variations of temperature on the vibratory periods of steel forks, I bought two sets of Kœnig's forks of the UT_2 harmonic series to known differences of temperature, and then determined how much they were thus thrown out of unison by the observation of the number of beats thus caused in one minute of time.

Instead of heating or cooling one set of the forks by automatic thermostats, which method had several objections in principle and great difficulties in the way of experimenting, I decided to wait for a favorable spell of weather, which we often have in April, when the air is still and misty and a drizzling rain occurs. In such weather the air is nearly constant in temperature. During such favorable conditions for the work, when the atmosphere varied only a few degrees in temperature during two days of mist and rain, I opened the windows of a room which contained one of the sets of forks and allowed them to remain there for a night and part of a day before beginning the experiments. In an adjoining room, kept at as nearly an equable temperature as possible, I placed the other forks. After the respective temperatures of these rooms had not varied perceptibly during three hours, I opened the door between the rooms just enough to hear clearly the forks of one room when stationed near the forks in the other. The temperature of the hot room was 66° Fahr., that of the other room was 41° Fahr.

Simultaneously sounding in order the two corresponding forks of the series, I obtained the

following results. The beats were timed with the aid of a stop-watch registering to one-tenth of a second. After each observation the door was closed and fifteen minutes allowed to elapse before beginning the observations on the two forks next in order. I should here remark, however, that the order in which the forks were experimented with was the reverse of that given in the table, that is to say, the experiments began with the UT_5 fork, of highest pitch; because the smaller mass of the higher forks would be most affected by any change of temperature from interchange of air of the rooms. I, however, observed no change in the temperature of the rooms during the experiments.

TABLE II.

The two UT_2 forks gave 11.6 beats in 60 seconds for a difference of 25° Fahr.
The two UT_3 forks gave 23.0 beats in 60 seconds for a difference of 25° Fahr.
The two SOL_3 forks gave 26.0 beats in 60 seconds for a difference of 25° Fahr.
The two UT_4 forks gave 32.5 beats in 60 seconds for a difference of 25° Fahr.
The two MI_4 forks gave 67.6 beats in 60 seconds for a difference of 25° Fahr.
The two UT_5 forks gave 81.5 beats in 60 seconds for a difference of 25° Fahr.

The forks in the cold room were a set recently received of KŒNIG; those in the warm room were a set of his forks which had been in constant use for several years and had become worn and somewhat rusted. To ascertain the difference in the numbers of vibrations of corresponding forks of the two sets, when at the same temperature, I had kept them for a day in the room which had the temperature of 66° Fahr., and after they had remained at this temperature during four hours we simultaneously sounded the two corresponding forks of the two sets with the following results:

TABLE III.

New UT_2 fork gave 2.3 beats in 60 seconds, with old UT_2 fork. Old fork flat.
New UT_3 fork gave 5.0 beats in 60 seconds, with old UT_3 fork. Old fork flat.
New SOL_3 fork gave 2.0 beats in 60 seconds, with old SOL_3 fork. Old fork sharp.
New UT_4 fork gave no beats in 60 seconds, with old UT_4 fork. Old fork in unison.
New MI_4 fork gave 12.0 beats in 60 seconds, with old MI_4 fork. Old fork flat.
New UT_5 fork gave 12.0 beats in 60 seconds, with old UT_5 fork. Old fork flat.

Correcting the observations of the number of beats given in Table II by the determination of beats contained in Table III, we have the actual numbers of beats per minute given by the forks for a difference in temperature of 25° Fahr., if the fork had been strictly in unison when at the same temperature, as follows:

TABLE IV.

The two UT_2 forks gave 9.3 beats in 60 seconds for a difference of 25° Fahr.
The two UT_3 forks gave 18.0 beats in 60 seconds for a difference of 25° Fahr.
The two SOL_3 forks gave 28.0 beats in 60 seconds for a difference of 25° Fahr.
The two UT_4 forks gave 34.5 beats in 60 seconds for a difference of 25° Fahr.
The two MI_4 forks gave 45.6 beats in 60 seconds for a difference of 25° Fahr.
The two UT_5 forks gave 69.6 beats in 60 seconds for a difference of 25° Fahr.

From the above determinations it follows:

TABLE V.

—, +, 1° Fahr. gives UT_2 fork +, —, .00600 of a vibration per second.
—, +, 1° Fahr. gives UT_3 fork +, —, .01200 of a vibration per second.
—, +, 1° Fahr. gives SOL_3 fork +, —, .018666 of a vibration per second.
—, +, 1° Fahr. gives UT_4 fork +, —, .023000 of a vibration per second.
—, +, 1° Fahr. gives MI_4 fork +, —, .030400 of a vibration per second.
—, +, 1° Fahr. gives UT_5 fork +, —, .046333 of a vibration per second.

The above results may be reduced to a more general statement by giving the effect of a change of 1° Fahr. on the forks' vibratory period, as follows:

TABLE VI.

+ 1° Fahr. diminishes	UT_2 fork's vibratory period	($\frac{1}{128}$ second)	$\frac{1}{21433}$	part.
+ 1° Fahr. diminishes	UT_3 fork's vibratory period	($\frac{1}{256}$ second)	$\frac{1}{21433}$	part.
+ 1° Fahr. diminishes	SOL_3 fork's vibratory period	($\frac{1}{384}$ second)	$\frac{1}{21433}$	part.
+ 1° Fahr. diminishes	UT_4 fork's vibratory period	($\frac{1}{512}$ second)	$\frac{1}{22217}$	part.
+ 1° Fahr. diminishes	MI_4 fork's vibratory period	($\frac{1}{640}$ second)	$\frac{1}{21072}$	part.
+ 1° Fahr. diminishes	UT_5 fork's vibratory period	($\frac{1}{1024}$ second)	$\frac{1}{22100}$	part.

From Table VI it is seen that the effect of a change of temperature on the vibratory period is the same for all forks made of the same steel and similarly shaped. The differences among the fractions of a vibratory period are small and evidently owing to the necessary errors of observation. I have great confidence in the accuracy of this determination. The mean fraction of the vibratory period which one of Kœnig's forks gains or loses by a diminution or increase of 1° Fahr. is $\frac{1}{21561}$ part, or .00004638.

THE LAW OF THE RUNNING DOWN IN THE AMPLITUDE OF A FORK'S VIBRATION.

Twelve sheets were carefully taken of the traces of an UT_2 fork of 128 vibrations per second. The fork was vibrated with a bow and the cylinder turned as uniformly as possible by the hand. The seconds were marked off on the traces of the fork by the break circuit of the clock. At or near each second mark on the sheets was measured with a microscope micrometer the amplitude of the vibration. The whole number of the sheets furnished over two hundred measures, giving the connection between the time the fork had run and the amplitude of its vibration at the end of that time. A curve was then plotted giving their relations. Its discussion showed that it was a logarithmic curve, which has the following expression: $y=(1.119)^x$.

EFFECT OF THE SUPPORT OF A FORK AND OF THE SCRAPE OF ITS TRACING-STYLE ON ITS VIBRATORY PERIOD.

These experiments on the effects of the support and scrape of the fork were made in connection with Prof. Albert A. Michelson with special reference to the period of vibration of the fork he used in timing the rotation of the mirror he employed in his experiments on the velocity of light. The fork was an UT_3 of Kœnig.

TABLE VII.

No. 1.

Temp. 80° Fahr. 80—65=15

15×.012=.180=correction for temperature.

(1).... 0.3	(6)....1289.2	(10)....2303.5
(2).... 256.1	(7)....1535.3	(11)....2559.0
(3).... 511.7	(8)....1791.5	(12).... 2825.3
(4).... 767.9	(9)....2047.1	(13)....3071.0
(5)....1023.5		

(7)—(1)—6=255.83
(8)—(2)—6=255.90
(9)—(3)—6=255.90
(10)—(4)—6=255.93
(11)—(5)—6=255.92
(12)—(6)—6=256.01
(13)—(7)—6=255.95

Mean..............255.920
Corr. for temp.... +.180

256.100
Corr. for clock.... .028

256.072

No. 2.

Temp. 81° Fahr. 81—65=16

16×.012=.192

(1).... 0.6	(5)....1024.5	(9)....2048.6
(2).... 256.7	(6)....1280.6	(10)....2304.9
(3).... 512.4	(7)....1536.2	(11)....2560.2
(4).... 768.3	(8)....1792.3	

(6)—(2)—4=255.97
(7)—(1)—6=256.93
(8)—(2)—6=255.93
(9)—(3)—6=256.03
(10)—(4)—6=256.10
[(11)—(5)—6=255.95]?

Mean 255.962
Corr. for temp.... +.192

256.154
Corr. for clock.... —.028

256.126

TABLE VII—Continued.

No. 3.

Temp. 81° Fahr. 81—65=16 16×.012=.192

(1)....0.1	(6)..........	(10)....2302.1
(2)....254.5	(7)....1535.2	(11)..........
(3)..........	(8)....1790.3	(12)....2814.0
(4)....766.3	(9)....2047.0	(13)....3071.1
(5)....1023.5		

(7)—(1)—6=255.85
(8)—(2)—6=255.97
(9)—(1)—8=255.86
(10)—(4)—6=255.97
(12)—(4)—8=255.96
(13)—(5)—8=255.95

Mean 255.927
Corr. for temp ....+.192

256.119
Corr. for clock ....—.028

256.091

No. 4.

Temp. 75° Fahr. 75—65=10 10×.012=.120

(1)....0.3	(6)......	(10)....2307.2
(2)....258.4	(7)....1536.1	(11)....2560.0
(3)....512.1	(8)....1795.0	(12)....2819.3
(4)....771.1	(9)....2048.1	(13)....3072.3
(5)....1024.1		

(7)—(1)—6=255.97
(8)—(2)—6=256.10
(9)—(3)—6=256.00
(10)—(4)—6=256.02
(11)—(5)—6=255.97
(12)—(4)—8=256.02
(13)—(7)—6=256.03

Mean 256.016
Corr. for temp....+.120

256.136
Corr. for clock ....—.028

256.108

No. 5.

Temp. 75° Fahr. 75—65=10 10×.012=.120

(1)....0.5	(5)....1024.3	(9)....2048.2
(2)....253.6	(6)..........	(10)....2301.3
(3)....512.3	(7)....1536.5	(11)....2560.0
(4)....765.5	(8)....1789.5	

(7)—(1)—6=256.00
(8)—(2)—6=255.98
(9)—(3)—6=255.98
(10)—(4)—6=255.97
(11)—(5)—6=255.95

Mean..........255.976
Corr. for temp ..+.120

256.096
Corr. for clock..—.028

256.068

No. 6.

Temp. 75° Fahr. 75—65=10 10×.012=.120

(1)....0.7	(5)....1024.7	(9)....2048.7
(2)....258.6	(6)....1282.7	(10)....2306.6
(3)....512.7	(7)....1536.6	(11)....2560.6
(4)....770.5	(8)....1794.5	(12)....2818.9

(7)—(1)—6=255.98
(8)—(2)—6=255.98
(9)—(3)—6=256.00
(10)—(4)—6=256.02
(11)—(5)—6=255.98
(12)—(6)—6=256.03

Mean............255.998
Corr. for temp....+.120

256.118
Corr. for clock....—.028

256.090

No. 7.

Temp. 75° Fahr. 75—65=10 10×.012=.120

(1)....0.1	(5)....1023.7	(9)....2047.5
(2)....257.9	(6)....1281.6	(10)....2306.2
(3)....512.0	(7)....1536.0	(11)....2560.0
(4)....770.0	(8)....1794.0	(12)....2818.3

(7)—(1)—6=255.98
(8)—(2)—6=256.02
(9)—(3)—6=255.92
(10)—(4)—6=256.05
(11)—(5)—6=256.05
(12)—(6)—6=256.12

Mean 256.020
Corr. for temp....+.120

256.140
Corr. for clock....—.028

256.112

No. 8.

Temp. 76° Fahr. 76—65=11 11×.012=.132

(1)....0.0	(5)....1023.7	(9)....2048.1
(2)....254.1	(6)....1278.1	(10)..........
(3)....512.0	(7)....1536.2	(11)....2559.9
(4)....766.0	(8)....1790.1	

(7)—(1)—6=256.03
(8)—(2)—6=256.00
(9)—(3)—6=256.02
(8)—(4)—4=256.02
(11)—(5)—6=256.03

Mean 256.020
Corr. for temp....+.132

256.152
Corr. for clock....—.028

256.124

TABLE VII—Continued.

No. 9.

Temp. 81° Fahr.	81—65=16	16×.012=.192
(1).... 0.8	(5)....1024.3	(9)....2048.4
(2).... 257.2	(6)....1280.7	(10)....2304.0
(3).... 512.7	(7)....1536.3	(11)....2560.2
(4).... 768.9	(8)....1792.5	

(7)—(1)—6=255.92
(8)—(2)—6=255.88
(9)—(3)—6=255.95
(10)—(4)—6=255.85
(11)—(5)—6=255.98

Mean 255.916
Corr. for temp ... +.192

256.108
Corr. for clock ... —.028

256.080

No. 10.

Temp. 81° Fahr.	81—65=16	16×.012=.192
(1).... 0.6	(6)....1281.9	(10)....2305.7
(2).... 258.1	(7)....1535.9	(11)....2559.7
(3).... 512.6	(8)....1793.8	(12)....2817.3
(4).... 770.0	(9)....2047.5	(13)....3071.5
(5)....1024.1		

(7)—(1)— 6=255.88
(8)—(2)— 6=255.95
(9)—(3)— 6=255.82
(10)—(4)— 6=255.95
(11)—(5)— 6=255.93
(12)—(6)— 6=255.90
(13)—(1)—12=255.91

Mean 255.906
Corr. for temp.... +.192

256.098
Corr. for clock .... —.028

256.070

The mean value of the above-determined ten means is as follows:

(1)......256.072
(2)......256.126
(3)......256.091
(4)......256.108
(5)......256.068
(6)......256.090
(7)......256.112
(8)......256.124
(9)......256.080
(10)......256.070

256.094
Correction for effects of support and scrape. —.026

256.068 Number of vibrations of fork on resonant box at 65° Fahr.

The correction —.026 for the effect of support and scrape of style of fork was determined as follows:

The standard UT_3 fork was placed in the same support (*H* of Fig. 1) which held it while it made its record on smoked paper, but fork vibrated freely, that is, it did not trace its vibrations on the paper. Another similar UT_3 fork was screwed on its resonant box and its prongs loaded with wax till it made about five beats per second with first fork. The beats were counted by coincidences with the one-fifth second beats of a watch.

TABLE VIII.

Coincidences were marked at 32 seconds; 39 seconds; 43.5 seconds; 49 seconds; 54.5 seconds; 61.5 seconds.

61.5—32=29.5; 29.5—5=5.9=time of one interval between coincidences.

RÉSUMÉ.—(1)=5.9 seconds; (2)=6.2 seconds; (3)=6.2 seconds; (4)=6.2 seconds. Mean=6.13=time of one interval between coincidences.

In this time, the watch makes 6.13×5=30.65 beats, and the forks make 30.65+1=31.65 beats. Hence the number of beats per second is 31.65÷6.13=5.163.

We now made similar experiments to the above, with the difference that the standard UT_3 fork was allowed to make its trace on the smoked paper, as it did when we determined its rate of vibration.

TABLE IX.

Coincidences were marked at 59 seconds; at 4 seconds; at 10.5 seconds; at 17 seconds.

$77-59=18$; $18-3=6.0$=time of one interval.

RÉSUMÉ.—(1)=6.0 seconds; (2)=6.0 seconds; (3)=6.7 seconds; (4)=6.3 seconds; (5)=6.5 seconds; (6)=6.7 seconds; (7)=6.0; mean=6.31 seconds.

$$6.31\times 5=31.55 \qquad 31.55+1.00=32.55$$

$$\begin{array}{rr} 32.55-6.31= & 5.159 \\ \text{With fork free}= & 5.163 \\ \hline \text{Effect of scrape}= & -.004 \end{array}$$

Circumstances as in first case, except that both forks were on their resonant boxes.

TABLE X.

Coincidences were observed at 21 seconds; at 28 seconds; at 36 seconds; at 44 seconds; at 51 seconds; at 60 seconds.

$60-21=39$; $39-5=7.8$=time of one interval.

RÉSUMÉ.—(1)=7.8 seconds; (2)=7.1 seconds; (3)=7.6 seconds; (4)=7.4 seconds; (5)=7.2 seconds; mean=7.42 seconds.

$$\begin{array}{rr} 72.42\times 5= & 37.10 \\ + & 1.00 \\ \hline & 38.10 \\ \hline\hline 38.10-7.42= & 5.133 \\ \text{Above}= & 5.159 \\ \hline \text{Effect of support and scrape}= & -.026 \end{array}$$

From the experiments it appears that the effect of the work of the fork in tracing its record on the smoked paper covering the cylinder, is only —.004 of a vibration; a quantity so small as to be negligible, as will appear further on where we give the probable error of the mean determination of the numbers of vibrations per second of various forks.

The difference in the number of vibrations given by the fork when vibrating on its resonant box and when vibrating while screwed into the hard wooden support (*H*, Fig. 1), amounts to —.026 less .004, or —.022. This result was not anticipated, and it shows how careful should experimenters be in describing minutely the character of the support of the fork when they give the value of its vibratory period.

Determination of the numbers of vibration per second of European forks of various standards of pitch

[Sent me by Mr. Alexander J. Ellis, F. R. S.]

These forks were the *A* fork of 1789, of the Chapelle Versailles; the *A* fork of 1812, of the Conservatoire; the *A* fork of 1818, of the Theâtre Feydeau; the *A* fork of 1820, of the Tuilleries, and a *C* fork made by Marloye of Paris.

The determination of the pitch of these forks was made with special care, and these measures may be regarded as the limits of accuracy of our method, so far as I have been able to deal with it. The fractions of vibrations on the records were read off with a microscope-micrometer, and the corrections for temperature and rate of clock were carefully obtained.

TABLE XI.

[Chapelle Versailles (*A*) fork of 1789.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction + .0185 $(t-65)$.	Vibrations in one second of mean time at 65° F.
		° *F.*						
1	1	65.3	+5.2	.99721	394.495	395.599	+.005	395.604
	3	65.3	+5.2	.99721	394.543	395.647	+.005	395.652
	4	65.3	+5.2	.99721	394.536	395.640	+.005	395.645
2	1	65.0	+5.2	.99721	394.509	395.613	+.000	395.613
	2	65.0	+5.2	.99721	394.470	395.574	+.000	395.574
	3	65.0	+5.2	.99721	394.570	395.674	+.000	395.674
	4	65.0	+5.2	.99721	394.493	395.597	+.000	395.597
	5	65.0	+5.2	.99721	394.517	395.621	+.000	395.621
3	1	65.0	+5.2	.99721	394.512	395.616	+.000	395.616
	2	65.0	+5.2	.99721	394.502	395.606	+.000	395.606
	3	65.0	+5.2	.99721	394.507	395.611	+.000	395.611
	4	65.0	+5.2	.99721	394.470	395.574	+.000	395.574
	5	65.0	+5.2	.99721	394.495	395.599	+.000	395.599
								395.614

TABLE XII.

[Conservatoire (*A*) fork of 1812.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction + .020 $(t-65)$.	Vibrations in one second of mean time at 65° F.
		F.						
1	1	65	+5.30	.99721	438.128	439.354	.000	439.354
	2	65	+5.30	.99721	438.075	439.301	.000	439.301
2	1	64.75	+5.30	.99721	438.161	439.387	−.005	439.382
3	1	63.6	+5.30	.99721	438.134	439.360	−.028	439.332
4	1	63.6	+5.30	.99721	438.118	439.344	−.028	439.316
								439.337

TABLE XIII.

[Theatre Feydeau (*A*) fork of 1818.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction + .019 $(t-65)$.	Vibrations in one second of mean time at 65° F.
		F.						
1	1	63.4	+5.2	.99721	432.812	434.023	−.031	433.922
	2	63.4	+5.2	.99721	432.799	434.010	−.031	433.979
	3	63.4	+5.2	.99721	432.809	434.019	−.031	433.988
	4	63.4	+5.2	.99721	432.800	434.011	−.031	433.980
2	1	63.8	+5.2	.99721	432.847	434.058	−.023	434.035
	2	63.8	+5.2	.99721	432.818	434.029	−.023	434.006
	3	63.8	+5.2	.99721	432.844	434.055	−.023	434.032
	4	63.8	+5.2	.99721	432.825	434.036	−.023	434.013
3	1	64.5	+5.2	.99721	432.799	434.010	−.010	434.000
	2	64.5	+5.2	.99721	432.837	434.048	−.010	434.038
	3	64.5	+5.2	.99721	432.805	434.016	−.010	434.006
	4	64.5	+5.2	.99721	432.828	434.039	−.010	434.027
								434.008

TABLE XIV.

[Tuilleries (*A*) fork of 1820.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction + .019 (*t* − 65).	Vibrations in one second of mean time at 65° F.
		F.						
1	1	65.5	+4.9	.99721	421.589	422.769	.01	422.759
	2	65.5	+4.9	.99721	421.597	422.776	.01	422.766
	3	65.5	+4.9	.99721	421.626	422.806	−.01	422.796
2	1	65.8	+4.9	.99721	421.632	422.812	.015	422.797
	2	65.8	+4.9	.99721	421.636	422.816	.015	422.801
	3	65.8	+4.9	.99721	421.641	422.821	.015	422.806
3	1	65.5	+4.9	.99721	421.667	422.847	.01	422.837
	2	65.5	+4.9	.99721	421.609	422.788	.01	422.778
	3	65.5	+4.9	.99721	421.624	422.804	.01	422.794
								422.793

TABLE XV.

[Marloye (*C*) fork.]

Sheet.	Trace.	Temperature.	Clock rate, sidereal.	Clock factor to correct record for rate.	Record.	Record corrected for rate.	Temperature correction.	Vibrations in one second of mean time at 65° F.
		F.						
3	1	69.25	+3.48	.99723	255.167	255.876	+.051	255.927
4	1	69.5	+3.48	.99723	255.224	255.933	+.054	255.987
6	1	71	+3.48	.99723	255.187	255.896	+.072	255.968
	2	71	+3.48	.99723	255.120	255.829	+.072	255.901
7	1	60	+4.35	.99722	255.208	255.919	.057	255.862
	2	60	+4.35	.99722	255.219	255.930	.057	255.873
[illegible]	1	61	+5.00	.99721	255.212	255.926	.048	255.878
	2	61	+5.00	.99721	255.221	255.935	−.048	255.887
								255.910

The determinations of the number of vibrations per second of the *A* forks have to be corrected by +.044 for the effect of the weight of the tracing style. The correction was too small to be determined in the case of the *C* fork.

The separate determinations of the number of vibrations of the Chapelle Versailles fork and those of the Théâtre Feydeau fork are numerous enough to give some idea of the probable error of a single determination and of the error of the mean of the determinations when these are discussed by the method of least squares.

From this discussion it appears that for the Chapelle Versailles fork, the probable error of a single determination $= \pm .019$ of a vibration; the probable error of the mean determination $= \pm .0053$ of a vibration.

From the experiments on the Théâtre Feydeau fork, the probable error of a single determination $= \pm .014$ of a vibration; the probable error of the mean determination $= \pm .004$ of a vibration.

These results show that the method is quite accurate, and certainly sufficiently so for the determination of the pitch of a standard fork, and for all purposes when the fork is used as a chronoscope in the measure of small intervals of time. If the error of the determination of the pitch of these two forks—when corrected for effects of support and scrape, which is a constant

readily determined—should only equal $\frac{5}{1000}$, or $\frac{1}{200}$ of a vibration in one second, a variation of that amount would be produced by a change of temperature of only one-fourth of a degree F. in the Théâtre Feydeau fork, and if measured in beats would amount to the difference in the pitch of two forks, which, when sounded together, would give one beat in 200 seconds.

ON THE USES OF THE TUNING-FORK AS A CHRONOSCOPE.—Various forms of chronoscopic apparatus contain a vibrating fork as a register of time. The majority of these are costly, by reason of the attempts of the inventors to obtain regular rotations of cylinders or disks by means of clock-work, when really all such appliances are useless. The fork itself, if only allowed to register its own trace on the revolving cylinder or disk, will give all that is desired without such adjuncts, for the accuracy of its registration has no connection with the rotation of the cylinder on which it leaves its record, and it matters not whether the latter be revolved quicker or slower, regularly or irregularly, so long as the motion is appreciably uniform during the trace of one flexure of the fork: this duration in the case of an UT_3 fork would be only the $\frac{1}{256}$ of a second, and in that minute interval it would not be possible to get a measurable variation in velocity unless we did our best to attain it. Any ordinary care in the rotation of the cylinder by hand will give waves which at and near the spark-mark will be found to be similar and equal, and therefore no error can be made in the measure of the fraction of a wave.

The numbers of vibrations of a fork per second can be determined to $\frac{1}{200}$ of a vibration, or, to be surely within bounds, say to the $\frac{1}{100}$ of a vibration, by the method we have described in this paper. This will give the time record with an A fork of 440 vibrations per second to $\frac{1}{44000}$ of a second.

It is not necessary to make any correction for the effect of the scrape or weight of tracing style or for the effect of the kind of support of the fork, for the number of the fork's vibrations per second is determined while the fork is on the same support it has when used as a chronoscope and while the fork is making its record; in other words, the number of the fork's vibrations per second are determined *in the exact conditions in which it is used as a chronoscope.*

The arrangement of such a chronoscope is of the simplest character. Fig. 4 shows it. As an example, we will suppose that we are to determine the initial velocity of a rifle-ball. *B* is a voltaic cell, whose current goes through the primary coil of the inductorium *I*, then to the target *T* formed of a metal plate (or a screen of wire, if we are determining the velocity of a cannon ball). This plate is very slightly inclined forwards, so that its upper edge presses very slightly against an adjusting screw at *S*. The abutting surfaces of this screw and the plate are amalgamated to insure good elastic contact. The bottom of the plate rests in a small trough of mercury. The current passes to this trough and out of the plate at the adjusting screw *S*, thence to the make-circuit lever *MC*, and back to the battery *B*. One pole of the secondary circuit of the inductorium is connected with the fork *F*, the other pole with the rotating cylinder *C*. The make-circuit lever is formed in this manner: It moves around a center at *O*. On its lower side are two platinum lugs. By the motion of the lever around *O*, either one or the other of these lugs are brought in contact with two platinum contact-pieces, *e* and *c*, which are insulated from the plate and standard on which the lever is supported.

The chronoscopic apparatus having been arranged as in the diagram, the fork is raised on the hinge *h* (see Fig. 1) and vibrated with a bow. The cylinder is revolved and the fork brought down on its smoked-paper surface. At the word "fire," the rifle is discharged. The fine wire or thread *w* is cut by the ball, and the weight *p* which it supported and which brought the left hand platinum lug onto the left hand insulated contact-piece, falls; then the spring *s* (or, better, a rubber band), which opposed the action of the weight, swings the right hand lug on to the right hand contact-piece. When the ball cut the wire, the primary circuit of the inductorium was broken, and a spark, at that instant, passed from the style of the fork and made a spark-hole in its sinuous trace. But the spring *s* at once made contact again, and the circuit was made through the right-hand lug *e*. The ball, therefore, reaches the target-plate *T* with the circuit closed, and when it strikes *T* the plate is thrown from the contact-screw *S*, and a second break takes place in the primary circuit and another spark passes from the style of the fork. By counting the number of waves and measuring with a microscope-micrometer the fraction of the

wave in the trace of the fork, we have the time it took the ball to go over the known distance from the wire *w* to the target *T*.

As an example of such work, we here give experiments we made on the velocity of the rifle-ball of .45 inch caliber of the United States Army cartridge. This ball weighs 405 grains, and the powder driving it weighs 70 grains.

TABLE XVI.

Number of experiment.	Waves between spark-holes.	Time in going over 60 feet.	Velocities per second.	Differences.
		Seconds.	*Feet.*	*Feet.*
(1)	11.31	.04418	1,358.0	+0.7
(2)	11.34	.04429	1,354.7	—2.6
(3)	11.30	.04411	1,359.3	+2.0
(4)	11.28	.04406	1,361.7	+4.4
(5)	11.35	.04433	1,353.3	—4.0
(6)	11.32	.04421	1,357.1	—0.2
			1,357.3	

The fifth column gives the differences of the separate determinations, and 1,357.3 feet the mean velocity of the ball per second. The average difference amounts to only 2.3 feet.

EXPERIMENTS WITH THE CHRONOSCOPE ON THE VELOCITIES OF FOWLING-PIECE SHOT OF VARIOUS SIZES PROJECTED WITH VARIOUS CHARGES OF POWDER FROM 12 AND 10 GAUGE GUNS.

The guns used in these experiments were "choke-bore," of the Colt Arms Manufacturing Company, of Hartford, Conn. They had rebounding locks. The primary current of the inductorium passed through a break-piece fixed under the rebounding hammer, so that at the instant the cartridge was exploded the electric current in the primary circuit of the inductorium was broken and then immediately formed again. The current which passed through this break-piece was led by a wire to an upright piece of tin plate, whose front surface leaned against a thick copper wire. Another wire led from the tin plate (which stood in a shallow trough of mercury) back to the battery.

The following tables give the results of our experiments:

TABLE XVII.

[10-gauge Colt gun; 5 drams Curtis & Harvey powder; 1¼-ounce shot.]

Size of shot.	Velocity 30 yards.	Velocity 40 yards.	Velocity 50 yards.
No. 1 Buck	1153	1067	....
FF	1147	1132	....
BB	1146	1126	....
No. 3	1066	1015	928
No. 6	1012	963	859
No. 8	995	880	775
No. 10	908	803	716

TABLE XVIII.

[10-gauge Colt gun: 4 drams Curtis & Harvey powder; 1¼-ounce shot.]

Size of shot.	Velocity 30 yards.	Velocity 40 yards.	Velocity 50 yards.
No. 1 Buck	1067	1018	....
FF	1017	1009	967
BB	1000	967	897
No. 3	989	911	872
No. 6	966	883	806
No. 8	920	874	776
No. 10	848	756	669

TABLE XIX.

[12-gauge Colt gun; 3¼ drams of Curtis & Harvey powder; 1¼-ounce shot.]

Size of shot.	Velocity 30 yards.	Velocity 40 yards.	Velocity 50 yards.
BB	862	795	667
No. 3	844	754	696
No. 6	825	739	600
No. 8	816	749	607
No. 10	796	680	610

TABLE XX.

[12-gauge Colt gun; 4 drams Curtis & Harvey powder; 1¼-ounce shot.]

Size of shot	Velocity 30 yards.	Velocity 40 yards.	Velocity 50 yards.
No. 8	847	722	671
No. 10	748	657	596

Each measure of velocity given in these tables is the mean value obtained from several experiments, varying in number from three to six. The headings "velocity 30, 40, 50 yards," mean that the numbers under them give the average velocities of the flight of shot over those distances, and not the velocities at 30, 40, and 50 yards from the gun.

It will be observed that the shot used were Nos. 10, 8, 6, 3, BB, FF, and No. 1 Buck. They were so selected because a pellet of any number in the above series weighs nearly double the preceding one. Thus a pellet of No. 8 weighs double one of No. 10, a pellet of No. 6 weighs double one of No. 8, and so on. These relations of weight among the pellets were obtained so that I could readily reach the relations existing between the velocities and the weights of pellets. The shot used was kindly furnished me by Tatham & Bros., of New York, who used carefully gauged sieves in their manufacture. The powder used was Curtis & Harvey's Diamond Grain No. 6. The powder and shot in each cartridge had been carefully weighed out in an accurate balance.

A glance at the tables at once shows the rapid increase in the velocity of the shot from No. 10 up to No. 3. With the heavier pellets the increase is less marked. Thus the table headed "10 Colt gun; 4 drams, Curtis & Harvey, 1¼ shot," shows that No. 8 shot has 72 feet per second velocity over No. 10 shot, and No. 6 has 46 feet over No. 8, while No. 3 has only 23 feet over No. 6, and BB shot gains only 11 feet over No. 3.

The relations between velocity and weight of pellet shown in this table may be taken as a type of all the experiments, and I have graphically shown their relations in the accompanying curve.

The divisions on the scale, measured on the axis of ordinates, give the velocity per second of the pellets. One unit on this axis equals 20 feet, and a unit on the axis of abscissas equals one unit of weight of pellet. The weight of a pellet of No. 10 shot is here taken as the unit of weight. The numbers of the shot are written under the axis of abscissas, the velocities along the axis of ordinates.

My friend Professor Rice, of the United States Naval Academy, who had previously made similar experiments with a Le Boulengé chronoscope, and who took great interest in these experiments, found that the curve here given is very nearly the curve of secants, and the formula for it is:

$$\frac{y}{b} = \sec.^{-1} \frac{x^n}{a}$$

where x is the velocity and y the weight of a pellet, and a b and n undetermined constants.

So far as the experiments with these two special guns show, there is a marked superiority in the 10 over the 12 gauge, when each is loaded with the same weight of powder and shot. Thus, with the same charges, viz, 4 drams powder and $1\frac{1}{4}$ ounces of shot fired from the 10 gauge, gives a velocity of 100 feet per second more than that given by the 12-gauge gun. This fact is conclusively shown in the comparison of the figures in the two tables XVIII and XX, and the difference in velocities is in favor of the 10 gauge in each of the sixty experiments which were made to get the numbers contained in the lines opposite No. 8 and No. 10.

With No. 10 shot the mean velocity given by the 10 gauge gun over the first 30 yards is 848 feet. With the same charge in the 12 gauge the velocity is 748 feet; showing a difference of 100 feet in favor of the 10 gauge. With No. 8 shot the experiments show a difference of 72 feet. The average difference in favor of the 10 gauge in the flight of shot Nos. 8 and 10 over 40 yards amounts to 110 feet.

If we assume, as we may without grave error, that the penetration of shot varies as the square of its velocity, these experiments will give the relative penetrations of the 10 to the 12 gauge gun about as 9 is to 7.

That the 10-gauge gun shows such marked superiority over the 12 may be accounted for by the fact that the same charge occupies less length in a 10 than in a 12-gauge, and hence there are fewer pellets in contact with the barrel of the former than of the latter to oppose by their friction the projectile force of the powder. Also, as these choke-bores are contracted two sizes at their muzzles, the action of the choke on the pellets in a 10-gauge, will, I think, be more effective than in the case of a 12, the pellets in the latter being more crowded together and conflicting in their actions than in the case of their discharge from a 10 bore. Also, some effect in favor of the 10-gauge may be owing to the fact that in this gun the powder is exploded nearer the center of the charge, and thus there is less chance of it blasting before it unburnt powder contained in the portion of the charge removed from the point of ignition.

I also venture to predict that with the same weight of barrels the 10-gauge will not heat as much as the 12, because the motion of the shot lost in the 12-gauge must appear in the form of heat.

The simplicity and inexpensiveness of the chronoscope we have described in this paper, its accuracy, and the ease with which it is used must commend it to all who will give it a trial under the conditions of its action which we have endeavored to set forth in this paper. Another of its advantages is that its records on the paper covering the cylinder are easily rendered permanent by drawing the unsmoked side of the paper over the surface of a dilute solution of photographic negative varnish contained in a wide shallow dish. On the records may be written with a blunt style the nature of the experiments they record before the carbon is fixed by the varnish, and then they can be bound togetner in book-form for preservation and reference.

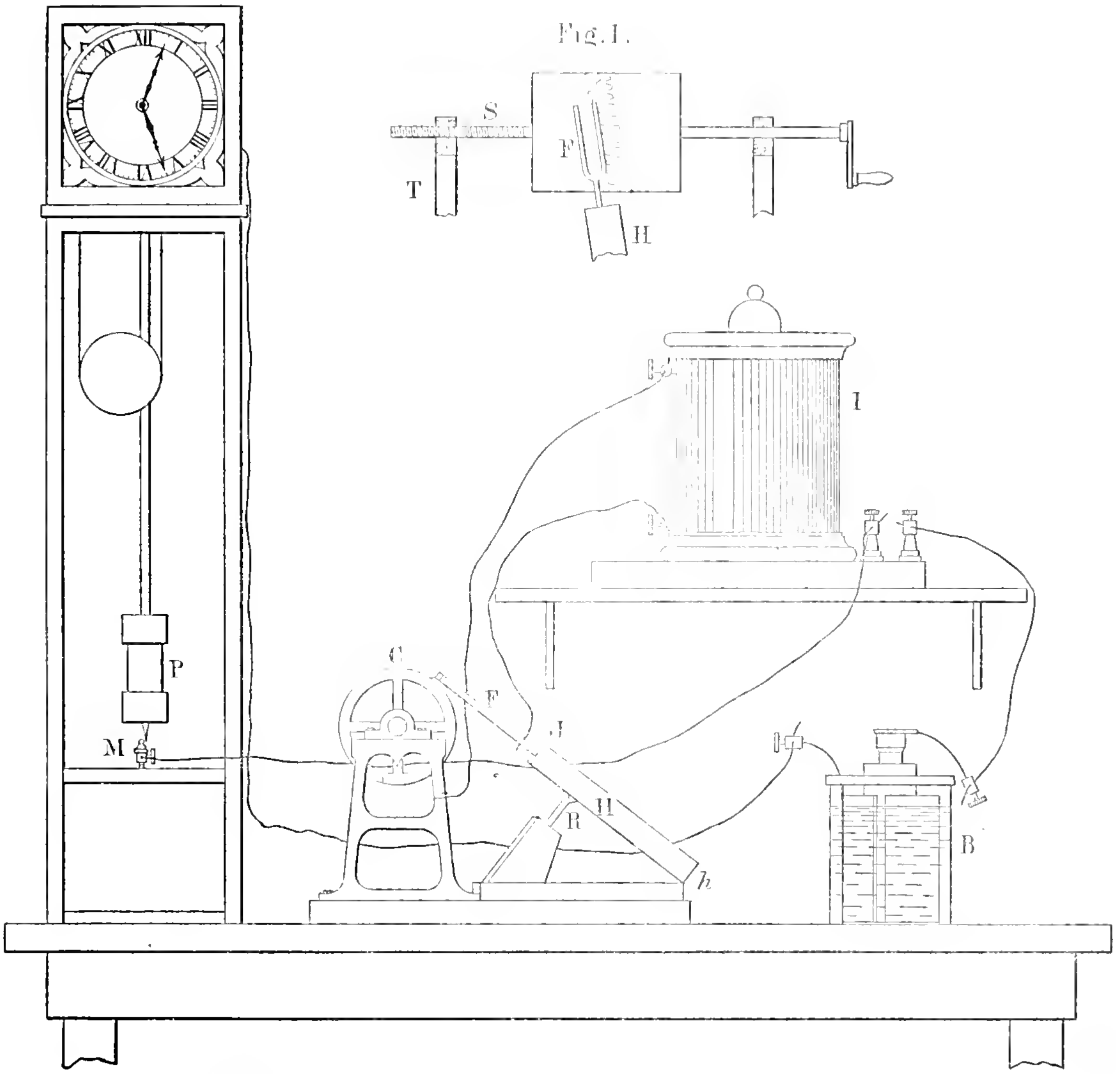
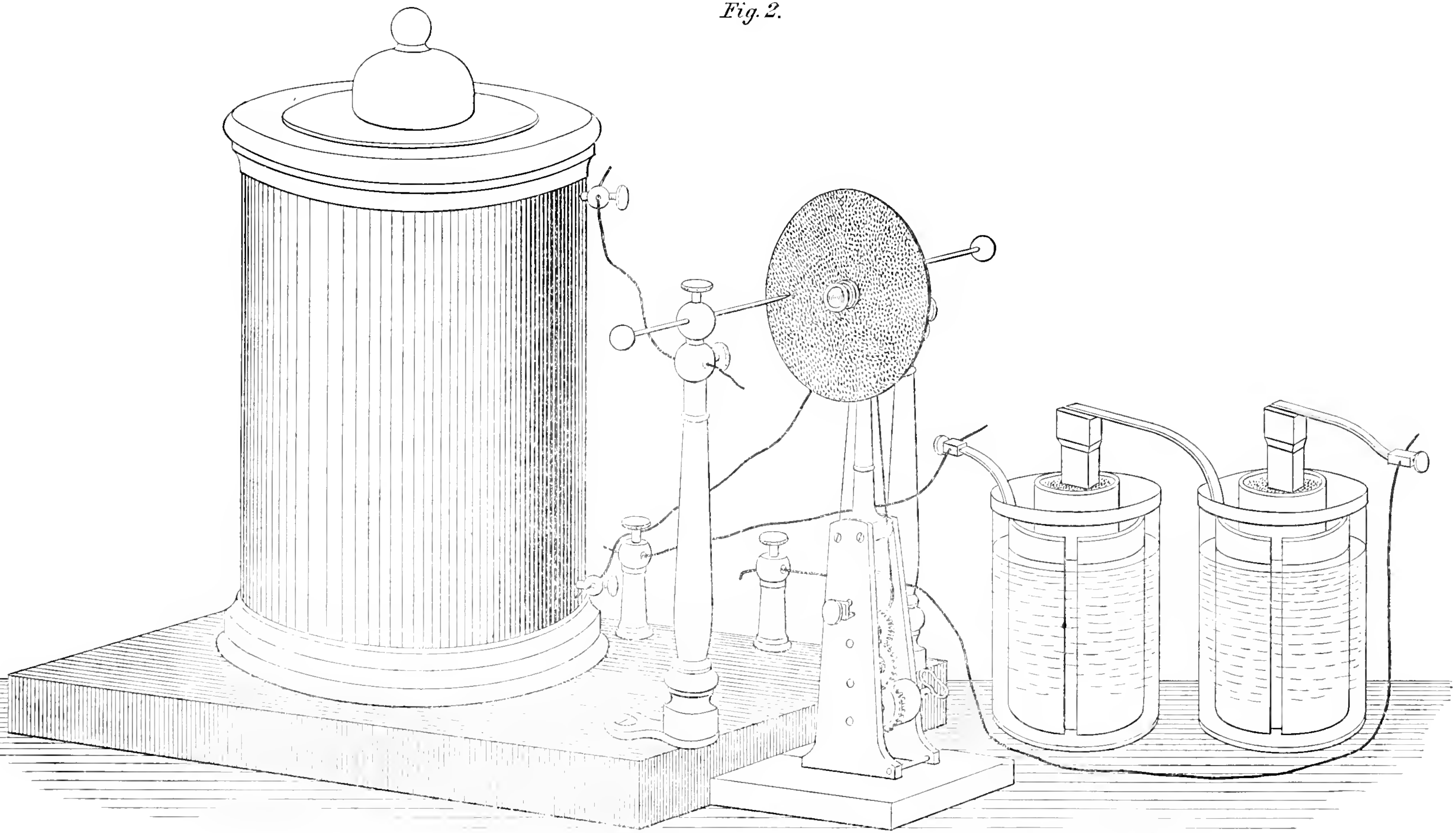


Fig. 1.
S
F
T
H
I
C
F
J
P
M
H
R
h
B

Fig. 2.



S. Mis. 69

Fig. 3.

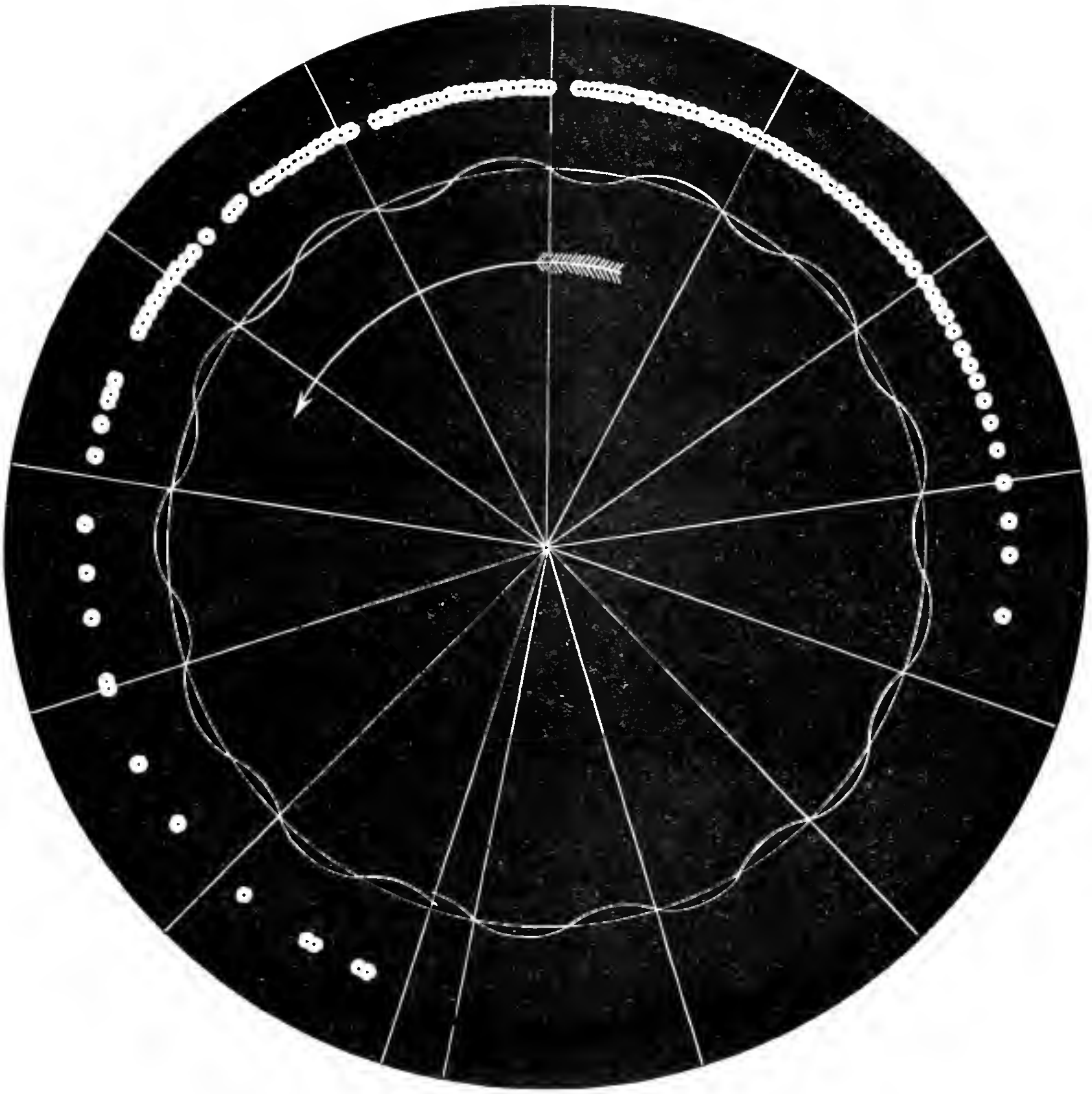


Fig. 4.

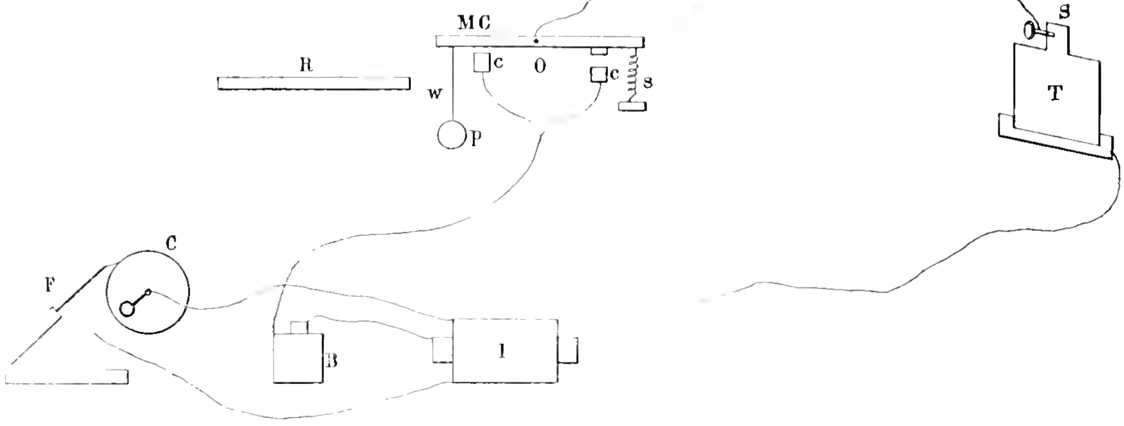
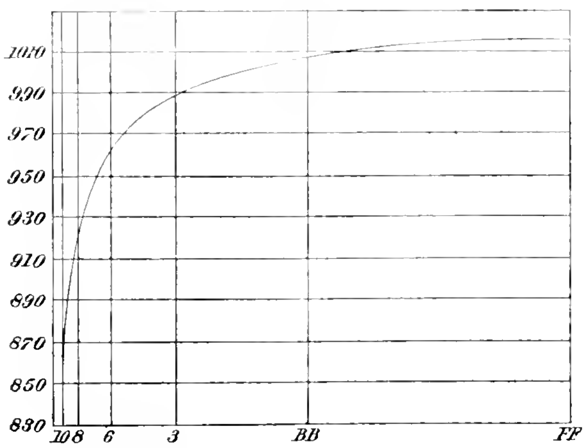

MC
R
c
O
c
s
w
P
S
T
C
F
B
I

Fig 5.


1010
990
970
950
930
910
890
870
850
830
10 8
6
3
BB
FF

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

FOURTH MEMOIR.

THE BAUMÉ HYDROMETERS.

THE BAUMÉ HYDROMETERS.

READ AT THE PHILADELPHIA MEETING, 1881.

By C. F. Chandler.

In 1768, Antoine Baumé, a chemist in Paris, published an account of two new instruments which he had devised for determining the specific gravity of liquids.

These instruments met with speedy acceptance on the part of practical men, and are now more extensively used in manufacturing establishments than any others.

Acids, alkalies, sugar solutions, petroleum oils, &c., are almost exclusively described in degrees Baumé.

The degrees on the Baumé scale are entirely arbitrary, and bear no obvious relation to the specific gravity of the liquid.

Baumé's hydrometers are instruments of even divisions. The special recommendation which has led to their extensive use among practical men is the simplicity of the numbers representing the specific gravity of the liquid. For liquids heavier than water the entire range is from zero to about 70 degrees. For liquids lighter than water, 10 to 80.

The numbers, therefore, are very easy to remember, and far more convenient on that account than the number expressing the true specific gravity, which for a liquid heavier than water would be 1 and a decimal of three figures usually, as for example 1.237.

Although Baumé described with great accuracy the method which he employed for securing the scale for his hydrometers, and it would seem, therefore, as though no difficulty existed to prevent the reproduction of his instruments, nevertheless it is a fact that among instrument-makers the scale has been so far modified from time to time that we have the greatest variety of instruments purporting to be Baumé's, each one of which has a set of degrees of an entirely different value from that exhibited by any other.

I have found twenty-three different scales, published by as many different writers, for liquids heavier than water, the highest of which gives as the value of 66° Baumé 1.8922; the lowest 1.730, no one of which can be said to be correct, or to have been obtained by following Baumé's directions.

For liquids lighter than water I have found eleven scales in which the value of 47° Baumé varies from 0.7978 to 0.7909.

Baumé's directions for the construction of his instruments are very simple, and it is almost incredible that such deviations should have occurred in connection with the instruments.

It has often been suggested that the only safe plan is to abandon the use of them entirely, and rely upon instruments which record at once the true specific gravity, referred to that of water as a unit.

The answer to this is that practical men will not abandon them, having become wedded to them, and preferring them on account of the simplicity of the numbers involved, and it would be impossible to induce them to give them up.

TABLE I.—*Value of degrees Baumé for liquids heavier than water, given by different authors.*

Degrees Baumé.	(1) Delezennes, 12.5° C. Mod.=139.97. Before 1848.	(2) Ziurek. Mod.=143.64. 1863.	(3) D'Arcet, 15° C. Mod.=143.68. 1816.	(4) Gilpin, 12.5° C. Mod.=143.83. 1797.	(5) French Codex, Holland. 12.5° C. Mod.=144. 1805.	(6) H. A. Mott, jr. 17.5° C. Mod.=144. 1877.	(7) Dalton. Mod.=144.01.	(8) Bourgougnon. 15° C. Mod.=144.32. 1878.	(9) Bineau. 15° C. Mod.=144.32. Before 1877.	(10) Vauquelin. 15° C. Mod.=144.38. 1810.	(11) Morozeau. 12.5° C. Mod.=144.38. 1830.	(12) Custom in France. Mod.=144.38. 1872.	(13) J. Kolb. 15° C. Mod.=144.38. 1872.	(14) H. Pemberton. Mod.=145. 1851.	(15) M'f'g Chem. Ass'n. U. S. A., A. H. Elliott. 15.5° C. Mod.=145.04. 1877.	(16) Schuler & Pecher. 15.5° C. Mod.=145.13. 1828.	(17) Huss. Ed'b'gh Disp. Mod.=145.40. 1830.	(18) Gerlach. 17.5° C. Mod.=146.77. 1870.	(19) Chemiker Kalender. Berlin. Mod.=146.78. 1881.	(20) Baumé original scale, 15° C. Mod.=149.57. Calculated by Gerlach, 1870.	(21) Baudin. 15° C. Mod.=150.0. 1870.	(22) Francoeur. Mod.=152.05. Before 1848.	(23) Bohmenberger. 14.4° C. Mod.=156.38. Before 1865.
0	1.0000	1.000	.....	1.000	1.000	1.0000	1.0000	1.0000	.....	.....	1.000	1.000	1.000	1.0000	1.000	1.0000	1.0000	1.0000	1.000	1.0000	1.0000	1.000	1.000
1	1.0072	1.007	.....	.....	1.007	1.0070	1.0074	1.0069	.....	.....	1.008	.....	1.007	1.0069	1.005	1.0069	1.0070	1.0068	1.007	.....	.....	1.007	1.007
2	1.0145	1.014	.....	.....	1.014	1.0141	1.0140	1.0140	.....	.....	1.015	.....	1.014	1.0139	1.011	1.0139	1.0141	1.0138	1.014	.....	.....	1.013	1.013
3	1.0219	1.022	.....	1.020	1.022	1.0213	1.0206	1.0212	.....	.....	1.022	.....	1.022	1.0211	1.023	1.0211	1.0213	1.0208	1.020	.....	.....	1.020	1.020
4	1.0294	1.029	.....	.....	1.029	1.0286	1.0268	1.0285	.....	.....	1.029	.....	1.029	1.0283	1.029	1.0283	1.0286	1.0280	1.028	.....	.....	1.027	1.027
5	1.0370	1.036	.....	.....	1.036	1.0360	1.0336	1.0358	1.036	1.023	1.036	1.023	1.037	1.0357	1.036	1.0356	1.0360	1.0353	1.034	1.0346	1.0344	1.034	1.033
6	1.0448	1.044	.....	1.040	1.044	1.0435	1.0405	1.0433	.....	.....	1.043	.....	1.045	1.0431	1.043	1.0431	1.0435	1.0426	1.041	.....	.....	1.041	1.040
7	1.0526	1.052	.....	.....	1.052	1.0511	1.0477	1.0509	.....	.....	1.051	.....	1.052	1.0507	1.050	1.0506	1.0511	1.0501	1.049	.....	.....	1.048	1.047
8	1.0606	1.060	.....	.....	1.060	1.0588	1.0544	1.0586	.....	.....	1.059	1.055	1.060	1.0583	1.057	1.0583	1.0588	1.0576	1.057	.....	.....	1.056	1.055
9	1.0687	1.067	.....	1.064	1.067	1.0667	1.0614	1.0665	.....	.....	1.067	.....	1.067	1.0661	1.064	1.0661	1.0666	1.0653	1.064	.....	.....	1.063	1.062
10	1.0769	1.075	.....	.....	1.075	1.0746	1.0743	1.0744	1.075	1.076	1.075	1.076	1.075	1.0740	1.071	1.0740	1.0745	1.0731	1.072	1.0716	1.0713	1.070	1.069
11	1.0853	1.083	.....	.....	1.083	1.0827	1.0809	1.0825	.....	.....	1.083	.....	1.083	1.0820	1.086	1.0820	1.0825	1.0810	1.080	.....	.....	1.078	1.077
12	1.0937	1.091	.....	1.089	1.091	1.0909	1.0887	1.0906	.....	.....	1.091	1.090	1.091	1.0902	1.093	1.0901	1.0906	1.0890	1.088	.....	.....	1.085	1.084
13	1.1023	1.100	.....	.....	1.100	1.0992	1.0953	1.0989	.....	.....	1.099	.....	1.100	1.0984	1.100	1.0983	1.0988	1.0972	1.096	.....	.....	1.094	1.092
14	1.1111	1.106	.....	.....	1.108	1.1111	1.1090	1.1074	.....	.....	1.107	.....	1.108	1.1068	1.107	1.1067	1.1071	1.1054	1.104	.....	.....	1.101	1.099
15	1.1200	1.116	.....	1.114	1.116	1.1163	1.1165	1.1159	1.116	1.114	1.116	1.114	1.116	1.1153	1.114	1.1152	1.1155	1.1138	1.113	1.1115	1.1110	1.109	1.107
16	1.1290	1.125	.....	.....	1.125	1.1250	1.1246	1.1246	.....	.....	1.125	.....	1.125	1.1240	1.122	1.1239	1.1240	1.1224	1.121	.....	.....	1.118	1.115
17	1.1382	1.134	.....	.....	1.134	1.1339	1.1330	1.1335	.....	.....	1.134	.....	1.134	1.1328	1.136	1.1326	1.1326	1.1310	1.130	.....	.....	1.126	1.123
18	1.1475	1.143	.....	1.140	1.143	1.1429	1.1410	1.1424	.....	.....	1.143	1.141	1.142	1.1417	1.143	1.1415	1.1414	1.1398	1.138	.....	.....	1.134	1.132
19	1.1570	1.152	.....	.....	1.152	1.1520	1.1480	1.1516	.....	.....	1.152	.....	1.152	1.1507	1.150	1.1506	1.1504	1.1487	1.147	.....	.....	1.143	1.140
20	1.1666	1.162	.....	.....	1.161	1.1613	1.1549	1.1608	1.161	1.162	1.161	1.162	1.162	1.1600	1.158	1.1598	1.1596	1.1578	1.157	1.1543	1.1538	1.152	1.148
21	1.1764	1.171	.....	1.170	1.171	1.1707	1.1626	1.1702	.....	.....	1.170	1.170	1.171	1.1693	1.172	1.1691	1.1690	1.1670	1.166	.....	.....	1.160	1.157
22	1.1864	1.180	.....	.....	1.180	1.1803	1.1706	1.1798	.....	.....	1.180	1.179	1.180	1.1788	1.179	1.1786	1.1785	1.1763	1.176	.....	.....	1.169	1.166
23	1.1965	1.190	.....	.....	1.190	1.1901	1.1876	1.1895	.....	.....	1.190	1.190	1.190	1.1885	1.186	1.1883	1.1882	1.1858	1.185	.....	.....	1.178	1.174
24	1.2068	1.199	.....	1.200	1.199	1.2000	1.1956	1.1994	.....	.....	1.200	1.200	1.200	1.1983	1.201	1.1981	1.1981	1.1955	1.195	.....	.....	1.188	1.183
25	1.2173	1.210	.....	.....	1.210	1.2101	1.2108	1.2095	1.209	1.210	1.210	1.210	1.210	1.2083	1.208	1.2080	1.2082	1.2053	1.205	1.2007	1.2000	1.197	1.192
26	1.2280	1.221	.....	.....	1.221	1.2203	1.2184	1.2197	.....	.....	1.220	.....	1.220	1.2184	1.216	1.2182	1.2184	1.2153	1.215	.....	.....	1.206	1.201
27	1.2389	1.231	.....	1.230	1.231	1.2308	1.2334	1.2301	.....	.....	1.230	.....	1.231	1.2288	1.231	1.2285	1.2288	1.2254	1.225	.....	.....	1.216	1.211
28	1.2499	1.242	.....	.....	1.242	1.2414	1.2409	1.2407	.....	.....	1.241	1.241	1.241	1.2393	1.238	1.2390	1.2394	1.2357	1.235	.....	.....	1.225	1.220
29	1.2612	1.252	.....	.....	1.252	1.2522	1.2490	1.2514	.....	.....	1.252	.....	1.252	1.2500	1.254	1.2497	1.2502	1.2462	1.245	.....	.....	1.235	1.230
30	1.2727	1.261	.....	1.261	1.261	1.2632	1.2654	1.2624	1.262	1.260	1.263	1.260	1.263	1.2608	1.262	1.2605	1.2612	1.2569	1.256	1.2509	1.2499	1.245	1.239
31	1.2844	1.275	.....	.....	1.275	1.2743	1.2740	1.2735	.....	.....	1.274	1.273	1.274	1.2719	1.269	1.2716	1.2724	1.2677	1.267	.....	.....	1.256	1.249
32	1.2962	1.286	.....	.....	1.286	1.2857	1.2826	1.2849		.....	1.285	1.284	1.285	1.2831	1.285	1.2828	1.2838	1.2788	1.278	.....	.....	1.267	1.260
33	1.3083	1.298	.....	1.295	1.298	1.2973	1.2999	1.2964	1.296	.....	1.296	1.296	1.297	1.2946	1.293	1.2943	1.2954	1.2901	1.289	.....	.....	1.277	1.270
34	1.3207	1.309	.....	.....	1.309	1.3091	1.3080	1.3081	.....	.....	1.308	1.307	1.308	1.3063	1.309	1.3059	1.3072	1.3015	1.300	.....	.....	1.288	1.281
35	1.3333	1.321	.....	.....	1.321	1.3211	1.3255	1.3201	1.320	1.315	1.320	1.315	1.320	1.3181	1.317	1.3177	1.3190	1.3131	1.312	1.3055	1.3042	1.299	1.291
36	1.3461	1.334	.....	1.333	1.334	1.3333	1.3345	1.3323	1.332	.....	1.332	1.329	1.332	1.3302	1.334	1.3298	1.3311	1.3250	1.324	.....	.....	1.310	1.302
37	1.3592	1.346	.....	.....	1.346	1.3458	1.3440	1.3447	1.345	.....	1.345	1.339	1.345	1.3425	1.342	1.3421	1.3434	1.3370	1.337	.....	.....	1.321	1.313
38	1.3725	1.359	.....	.....	1.359	1.3585	1.3612	1.3574	1.357	.....	1.358	1.359	1.357	1.3551	1.350	1.3546	1.3559	1.3494	1.349	.....	.....	1.333	1.325
39	1.3861	1.372	.....	1.373	1.372	1.3714	1.3788	1.3703	1.370	.....	1.371	1.372	1.370	1.3679	1.368	1.3674	1.3686	1.3619	1.361	.....	.....	1.345	1.336
40	1.3999	1.384	.....	.....	1.384	1.3846	1.3884	1.3834	1.383	1.375	1.384	1.375	1.383	1.3809	1.386	1.3804	1.3815	1.3746	1.375	1.3650	1.3635	1.357	1.347
41	1.4141	1.398	.....		1.398	1.3981	1.3977	1.3968	1.397	.....	1.397	1.399	1.397	1.3942	1.395	1.3937	1.3947	1.3876	1.388	.....	.....	1.369	1.359
42	1.4285	1.412	.....	1.414	1.412	1.4118	1.4170	1.4104	1.410	.....	1.410	1.413	1.410	1.4077	1.413	1.4072	1.4082	1.4009	1.401	.....	.....	1.381	1.371
43	1.4433	1.426	.....	.....	1.426	1.4267	1.4265	1.4244	1.424	.....	1.424	1.427	1.424	1.4215	1.422	1.4210	1.4219	1.4143	1.414	.....	.....	1.395	1.384
44	1.4583	1.440	.....	.....	1.440	1.4400	1.4460	1.4386	1.438	.....	1.438	1.441	1.438	1.4356	1.441	1.4350	1.4359	1.4281	1.428	.....	.....	1.407	1.396
45	1.4735	1.454	1.454	1.455	1.454	1.4545	1.4560	1.4530	1.453	1.466	1.453	1.455	1.453	1.4500	1.451	1.4493	1.4501	1.4421	1.442	1.4303	1.4204	1.420	1.404
46	1.4893	1.470	1.466	.....	1.470	1.4694	1.4660	1.4678	1.468	.....	1.468	1.466	1.468	1.4646	1.470	1.4640	1.4645	1.4564	1.456	.....	.....	1.434	.....
47	1.5053	1.485	1.482	.....	1.485	1.4845	1.4860	1.4829	1.483	.....	1.483	1.482	1.483	1.4795	1.480	1.4789	1.4792	1.4710	1.470	.....	.....	1.448	.....
48	1.5217	1.501	1.500	1.500	1.501	1.5000	1.4960	1.4983	1.498	.....	1.498	1.500	1.498	1.4949	1.500	1.4941	1.4942	1.4860	1.485	.....	.....	1.462	.....
49	1.5384	1.516	1.515	.....	1.516	1.5158	1.5170	1.5140	1.514	.....	1.514	1.515	1.514	1.5104	1.510	1.5097	1.5096	1.5012	1.500	.....	.....	1.476	.....
50	1.5555	1.532	1.532	.....	1.532	1.5319	1.5280	1.5301	1.530	1.524	1.530	1.532	1.530	1.5263	1.531	1.5255	1.5253	1.5167	1.515	1.5021	1.5000	1.490	.....
51	1.5730	1.549	1.550	1.547	1.549	1.5484	1.5503	1.5465	1.546	.....	1.546	1.550	1.540	1.5425	1.541	1.5417	1.5413	1.5325	1.531	.....	.....	1.505	.....
52	1.5909	1.566	1.566	.....	1.566	1.5652	1.5648	1.5632	1.563	.....	1.563	1.566	1.563	1.5591	1.562	1.5583	1.5576	1.5487	1.546	.....	.....	1.520	.....
53	1.6096	1.583	1.586		1.583	1.5824	1.5760	1.5802	1.580	.....	1.580	1.586	1.580	1.5760	1.573	1.5752	1.5742	1.5652	1.562	.....	.....	1.535	.....
54	1.6279	1.601	1.603	1.594	1.601	1.6000	1.5975	1.5978	1.597	.....	1.598	1.603	1.597	1.5934	1.594	1.5925	1.5912	1.5820	1.578	.....	.....	1.551	.....
55	1.6471	1.618	1.618	.....	1.618	1.6179	1.6090	1.6157	1.615	1.618	1.616	1.618	1.615	1.6111	1.616	1.6101	1.6086	1.5993	1.596	1.5816	1.5789	1.567	.....
56	1.6667	1.638	.....	.....	1.637	1.6363	1.6415	1.6340	1.634	.....	1.634	1.639	1.634	1.6292	1.627	1.6282	1.6264	1.6169	1.615	.....	.....	1.583	.....
57	1.6868	1.659	.....	1.659	1.656	1.6551	1.6520	1.6527	1.652	.....	1.653	1.660	1.652	1.6477	1.650	1.6467	1.6446	1.6349	1.634	.....	.....	1.600	.....
58	1.7074	1.678	.....	.....	1.676	1.6744	1.6750	1.6719	1.671	.....	1.672	1.682	1.671	1.6666	1.661	1.6656	1.6632	1.6533	1.653	.....	.....	1.617	.....
59	1.7285	1.698	.....	.....	1.695	1.6941	1.6993	1.6915	1.691	.....	1.691	1.703	1.691	1.6860	1.683	1.6849	1.6823	1.6721	1.671	.....	.....	1.634	.....
60	1.7501	1.718	1.717	1.717	1.715	1.7142	1.7120	1.7115	1.711	1.725	1.711	1.725	1.711	1.7058	1.705	1.7047	1.7019	1.6914	1.699	1.6698	1.6666	1.652	.....
61	1.7722	1.739	.....	.....	1.736	1.7349	1.7360	1.7321	1.732	.....	1.732	1.744	1.732	1.7261	1.727	1.7250	1.7220	1.7111	1.709	.....	.....	1.670	.....
62	1.7950	1.760	.....		1.758	1.7560	1.7570	1.7531	1.753	.....	1.753	1.764	1.753	1.7469	1.747	1.7457	1.7427	1.7313	1.729	.....	.....	1.689	.....
63	1.8184	1.782	.....	1.779	1.779	1.7777	1.7774	1.7748	1.774	.....	1.775	1.783	1.774	1.7682	1.767	1.7669	1.7640	1.7520	1.750	.....	.....	1.708	.....
64	1.8423	1.804	.....	.....	1.801	1.7999	1.8048	1.7968	1.796	.....	1.797	1.803	1.796	1.7901	1.793	1.7888	1.7858	1.7731	1.771	.....	.....	1.727	.....
65	1.8669	1.827	.....		1.823	1.8227	1.8233	1.8194	1.819		1.819	1.822	1.819	1.8125	1.814	1.8111	1.8082	1.7948	1.793	1.7685	1.7646	1.747	.....
66	1.8922	1.850	1.849	1.848	1.847	1.8461	1.8460	1.8427	1.8426	1.842	1.842	1.842	1.842	1.8354	1.835	1.8340	1.8312	1.8171	1.815	1.7897	1.786	1.767	*1.730
67	1.9180	1.874	.....	.....	1.872	1.8701	.....	1.8665	.....	.....	1.866	.....	.....	1.8589	.....	1.8574	1.8548	1.8398	1.839	.....	.....	1.788	.....
68	1.9447	1.898	.....	.....	1.897	1.8947	.....	1.8909	.....	.....	1.891	.....	.....	1.8831	.....	1.8815	1.8790	1.8632	1.864	.....	.....	1.809	.....
69	1.9721	1.922	.....	1.920	1.921	1.9200	.....	1.9161	.....	.....	1.916	.....	.....	1.9079	.....	1.9062	1.9038	1.8871	1.885	.....	.....	1.831	.....
70	2.0003	1.947	.....	.....	1.946	1.9459		1.9418	.....	.....	1.942	.....	.....	1.9333	.....	1.9316	1.9291	1.9117	1.909	1.8796	1.8750	1.854	.....
71	.....	1.973	.....		1.974	1.9726	.....	1.9683	.....	.....	1.968	.....	.....	1.9595	.....	1.9577	1.9548	1.9370	1.935	.....	.....	1.877	.....
72	.....	2.000	.....	2.000	2.002	2.0000	.....	1.9955	.....	.....	1.995	.....	.....	1.9863	.....	1.9844	1.9809	1.9629	1.960	.....	.....	1.900	.....
73	.....	.....	.....	.....	2.031	.....	.....	2.0235	.....	.....	2.023	.....	.....	2.0139	.....	2.0119	2.0073	1.9895	.....	.....	.....	1.924	.....
74	.....	.....	.....	.....	2.059	.....	.....	2.0523	.....	.....	2.052	.....	.....	2.0422	.....	2.0402	2.0340	2.0167	.....	.....	.....	1.949	.....
75	.....	.....	.....	.....	2.087	.....	.....	2.0819	.....	.....	2.081	.....	.....	2.0714	.....	2.0693	2.0610	2.0449	.....	.....	.....	1.974	.....
76	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	2.0992	.....	.....	.....	.....	.....	2.000	.....

*Calculated by the compiler.

NOTE.—Where the modulus was not given it was calculated by the formula: $n = \frac{P \cdot d}{P - 1}$ in which n = modulus; P = specific gravity = Baumé degree. 66 was taken for "d" whenever the corresponding Sp. Gr. appeared.

The only thing to be done is to correct the inaccuracies and establish by some competent authority an authorized and accepted standard of values for the Baumé scales.

Baume's methods were first described in *L'Avant Coureur* towards the close of 1768. They have been repeated in the several editions of his Élements de Pharmacie. In the eighth edition of this work, published in Paris in 1797, he states that he constructed his instruments in this way:

(1) *For the hydrometer for liquids heavier than water* he prepared a solution of salt containing fifteen (15) parts of salt by weight in eighty-five (85) parts of water by weight. He describes the salt as "very pure" and "very dry," and states that the experiments should be made in a cellar in which the temperature is 10° Reaumur, equivalent to 12.5° Centigrade and to 54.5° Fahrenheit. The zero on the scale indicates the point to which the instrument sinks in distilled water (at the temperature above stated), the 15 mark the point to which it sinks in the 15 per cent. salt solution. With a pair of dividers the space between "0" and "15" is divided into fifteen equal parts, and degrees of the same size are continued above "15."

Baumé's idea was that each additional degree on this scale would indicate one additional per cent. of salt, which of course is not quite correct, but the directions given are sufficiently simple to enable any person to reproduce the instrument.

(2) *For the hydrometer for liquids lighter than water* he uses a 10 per cent. solution of salt prepared in the same way, and by means of it fixes the zero point on the hydrometer. He uses distilled water for the "10" point, and obtains a scale as in the case of the other instrument, but running in the opposite direction.

With so simple and direct a statement as this it is remarkable that it has been possible to get so far away from the true Baume scales. In looking over the literature of the subject I find that these discrepancies have arisen from various causes—either neglect to follow Baumé's directions, or a deliberate attempt to improve the scales.

(*a*) Baumé conveys the idea that each degree represents 1 per cent. of salt, and he even suggests that in order to obviate errors due to irregularities in the stem of the instrument, a series of solutions may be prepared, the first containing 1 per cent. of salt and 99 per cent. of water, the second 2 per cent. of salt and 98 per cent. of water, and so on, and that the degrees 1, 2, &c., can be marked by the use of these solutions.

(*b*) Acting still further on this suggestion of Baumé, many instrument makers gave up preparing the 15 per cent. salt solution altogether for fixing the "15" mark, using instead the 10 per cent. solution and fixing by it the "10" mark, thus making one solution answer for both instruments.

(*c*) It was found at an early day that oil of vitriol generally stood at about 66 on the Baumé instrument; so many instrument makers fixed the 66 mark by immersing the instrument in oil of vitriol. As a matter of fact oil of vitriol is a variable substance. It never contains 100 per cent. of sulphuric acid—usually only from 92 to 96 per cent. It consequently has a variable specific gravity, and its use for the 66° mark introduces varying errors.

SCALES FOR LIQUIDS HEAVIER THAN WATER.

I submit herewith a table—"Table No. I"—containing twenty-three different scales of values for the degrees on the instrument for liquids heavier than water, and another table—"Table No. II"—containing the eleven scales for liquids lighter than water.

METHODS EMPLOYED IN SECURING THE SCALES GIVEN IN TABLE I.

(1) Delezennes. 66°=1.8922. The mark for 10° was found by a 10 per cent. salt solution at 10° R. (Wagner Jahresb., 1869, vol. 15, 236.) This scale appears in Journal de Phys., vol. 94, 204; Bache & McCulloh, 1848, 116; Dingler's Polyt. Journal, 1865, 2 vol., 176, 455; Handwörterbuch der Chemie, 1859, vol. 2, 1. 179; Knapp, Chem. Tech.

(2) Zinrek. 66°=1.850. No method given. This scale appears in Technologische Tabellen, 1863, 35.

(3) D'Arcet. 66°=1.849 (calculated). The point 66° B. was obtained in sulphuric acid of specific gravity 1.830, but it is assumed that it is not pure hydrate, but contains about 6 to 7 per cent.

more water than the hydrate H_2SO_4. (Muspratt, vol. 6, 357.) This scale appears in Bull. Soc. Ind. de Mülhouse, 1872; Muspratt, 1879, vol. 6, 359.

(4) Gilpin. 66°=1.848. The mark 10° was found by a 10 per cent. salt solution at 10° R. (Wagner, Jahresb., 1869, vol. 15, 236.) This scale appears in Henry, 1810; Children, 1819; Ann. de Chimie, vol. 23, 1797; Handwörterbuch, vol. 2, 1; Bache & McCulloh, 1848; Knapp, Chem. Tech., vol. 1, part 5; Journal de Physique, 1797.

(5) French Codex (Holland). 66°=1.847. In the Holland scale, the 10° was obtained by a 10 per cent. common salt solution at 10° R. (Bache & McCulloh. Reports on Sugar and Hydrometers, 1848, 84.) This scale appears in U. S. Dispensatory, 5th, 7th, 8th, 11th, 12th, 13th, and 14th editions; Pharmacopœa Batava, 1805; Bache & McCulloh, 1848; Neues Handwörterbuch, 1871; Dingler's Polyt. Journal, 1870.

(6) H. A. Mott, jr. 66°=1.8461. Was deduced by Doctor Pyle, of Philadelphia, and the table calculated to 0.5 by Doctor Mott. (Letter from Dr. M. to Dr. C. F. C., Nov. 8, 1881.) This scale appears in Mott, Chemist's Manual, 1877.

(7) Dalton. 66°=1.8460. The point 66° was obtained in sulphuric acid of specific gravity 1.830 (see D'Arcet). (Muspratt, 1879, vol. 6, 357.) This scale appears in Muspratt's Technische Chemie, 1879, vol. 6.

(8) Bourgougnon. 66°=1.8427. This table is calculated according to the formula—

$$P=\frac{144.3}{144.3-d}$$

in which P=density; d=degree Baumé. This formula is obtained when Gay-Lussac's method is used with sulphuric acid of specific gravity 1.8427 at 15° C. (Tucker, Manual of Sugar Analysis, 1881, pp. 108, 109.) This scale appears in Proc. Am. Chem. Soc., vol. 1, No. 5, 1878; Tucker, Manual of Sugar Analysis, 1881.

(9) Bineau. 66°=1.8426. In Bineau's tables, which Otto has calculated for 15° C. according to Bineau's own statements, the specific gravity of the sulphuric acid (Schwefelsaürehydrates) at 15° C. =1.8426. (Wagner, Jahresb., 1869, vol. 15, 238.) This scale appears in Muspratt, 1879, vol. 6, 358; Agendas Dunod, 1877; Lunge, 1879, vol. 1.

(10) Vauquelin. 66°=1.842. The point 66° was obtained in sulphuric acid of specific gravity 1.830 (see D'Arcet). (Muspratt, 1879, vol. 6, 357.) This scale appears in Ann. de Chimie et Physique, 1 series, vol. 76; Bull. Ind. de Mülhouse, 1872; Muspratt, 1879, vol. 6, 359.

(11) Morozeau. 66°=1.842. Calculated by Morozeau by the formula

$$y=\frac{dd'(n'-n)}{n'd'-nd-x(d'-d)}$$

n, n', and x are the degrees of the instrument corresponding to the specific gravities, d, d', and y. The 66°=1.842 at 10° R. This number is accepted because it corresponds to the highest specific gravity of "acide sulfurique hydreux," because it is given by Thénard and because it seems generally accepted. In giving to x the values 1, 2, 3, up to 75, the corresponding values of y have therefrom been deduced. (Journal de Pharmacie, Paris, 1830, vol. 16, p. 488.) This scale appears in Journal de Pharmacie, vol. 16, 488; Knapp, Chem. Technologie; École Centrale Lyonnaise.

(12) Custom in France. 66°=1.842. This table is based on Vauquelin's table. (Bull. Soc. Ind. de Mülhouse (42) 1872, p. 211.) This scale appears in Bull. Soc. Ind. de Mülhouse, 1872.

(13) J. Kolb. 66°=1.842. 66°=pure sulphuric acid of specific gravity 1.842. (Lunge Soda Industrie, 1879, vol. 1, 24.) This scale appears in Bull. Soc. Ind. de Mülhouse, 1872; Roscoe and Schorlemmer, 1877; Wurtz, Dict. de Chimie, 1876; Lunge, Soda Industrie, 1879, vol. 1; Deut. Chem. Kalendar. Dresden, 1877; Wagner, Chem. Tech., 1875; Muspratt, 1879, vol. 6, 359. Nos. 10, 11, 12, and 13 all give 66° Baumé=1.842, though differing in other terms.

(14) H. Pemberton. 66°=1.8354. Calculated by H. Pemberton in 1851, and adopted as standard by the Philadelphia College of Pharmacy the same year. This scale appears in U. S. Dispensatory, 12th, 13th, and 14th editions.

(15) Manufacturing Chemists' Association, U. S. A. 66° = 1.835. Calculated by A. H. Elliott from the data given by the committee on "What is oil of vitriol?" in 1875,

66° B =	H_2SO_4	93.5
	H_2O	6.5
		100.0

This scale appears in a separate sheet published by the association. In a report of the Commission on "What is oil of vitriol?" previously published, a table differing slightly from this is published.

(16) Schober and Pecher. 66° = 1.8340. The mark 10° was obtained by a 10 per cent. salt solution of specific gravity 1.074 and the scale calculated by the formula

$$S = \frac{10\,p}{10\,p + n\,(p-1)}$$

in which S = specific gravity of the fluid, p = specific gravity of salt solution, n = degrees. (Dingler's Polyt. J., 1828, vol. 27, 63.) This scale appears in Dingler's Polyt. J., vol. 27, 63; Hoffmann-Schaedler Tabellen, 1877; Knapp, Chem. Tech.; E. L. Schubarth, vol. 1, 47.

(17) Huss, Edinburgh Dispensatory. 66° = 1.8312. Calculated by Huss and published in Duncan's Ed'b'gh Disp., 1830. This scale appears in Duncan's Ed'b'gh Disp., 1830; U. S. Dispensatory, 5th, 7th, 8th, 11th, 12th, 13th, and 14th editions.

(18) Gerlach. 66° = 1.8171. Based on a 10 per cent. salt solution of specific gravity 1.07311 at 14° R. (Dingler Polyt. J., 1870, 198, 315.) This scale appears in Dingler's Polyt. J., 1870; Post, Chem. Tech. Analyse, 1881, Part 1, 438; Lunge, Soda Industrie, 1879, vol. 1.

(19) Chemiker Kalender. Berlin. 66° = 1.815. No method stated. This scale appears in Chem. Kalender, Berlin, Dr. Biedermann, 1881.

(20) "Baumé Original Scale." As calculated by Gerlach, 1870. 66° = 1.7897. Based on the specific gravity of a 15 per cent. salt solution *in vacuo* at 15° C. = 1.11146. This scale appears in Dingler's Polyt. J., 1870, vol. 198, 316.

(21) Baudin. 66° = 1.786 (calculated). A 15 per cent. salt solution of specific gravity 1.111 was employed for the 15 mark, at 15° C. (Chemical News, 1870, vol. 21, 51.) This scale appears in Chemical News, 1870, vol. 21, 54.

(22) Francœur. 66° = 1.767. The 15 mark was obtained by a 15 per cent. solution of rock salt dissolved in distilled water at maximum density specific gravity = 1.1094. (Francœur, Mémoire, sur l'Aréomètrie, 1842, Paris, 26.) This scale appears in Watts' Dict., vol. 3, 209; Johnson's Cycl., vol. 2, 1062; Fownes' Chemistry, 12th ed.; Ure's Dict., vol. 1; Handwörterbuch der Chemie, vol. 2, 1; Knapp, Chem. Tech.; Bache & McCulloh, 1848.

(23) Bohnenberger. 66° = 1.730 (calculated). Probably a 15 per cent. salt solution at 11.5° R. was employed for the 15 mark. (Wagner. Jahresb., 1869, vol. 15, 235.) This scale appears in Handwörterbuch der Chemie, vol. 2, 1; Practical Magazine; Dingler's Polyt. J., 1865, vol. 176; Tüb. Blätter, vol. 2, 457; Knapp, Chem. Technology.

THE TRUE SCALE FOR LIQUIDS HEAVIER THAN WATER.

As no one of these twenty-three scales had been obtained by following Baumé exactly, it was deemed advisable to repeat his experiments.

Three solutions were prepared by following exactly the directions of Baumé, each one containing 15 per cent. of salt and 85 per cent. of water by weight. For the first solution chemically pure sodium chloride was employed; for the second, "solar salt," from Syracuse; for the third, "factory-filled dairy salt," from Syracuse. The specific gravity of these solutions was carefully determined at 10° Reaumur. The results are given in Table III, together with the results obtained by several friends who have repeated this experiment, and also of several chemists who have published their results.

TABLE II.—*Value of degrees Baumé for liquids lighter than water, given by different authors.*

[Compiled by C. F. Chandler and F. G. Wiechmann, 1882.]

Degrees Baumé.	Francœur, 12.5 C. Mod. 145.98, 1842. (?)[1]	Gilpin, 12.5 C. Mod. 145.26, 1794.[2]	Chemiker Kalendar, 15.62 C. Mod. 145.26, 1881.[3]	Schuler & Pecher, 15.62 C. Mod. 145.17, 1828.[4]	Holland, 12.5 C. Mod. 144.37, 1865.[5]	French Codex, 12.5 C. Mod.=143.48, 1850, (?)[6]	Brix, Mod.=143.13, before 1865.[7]	Delezennes, 12.5 C. Mod.=140.11, before 1848.[8]	Huss, Ed'b'gh Disp. Mod.=140.11, 1830.[9]	Zimrek, 12.5 C. Mod. 140.03, 1863.[10]	Pemberton, Mod.=139.94, 1851.[11]
10	1.0000	1.000	1.000	1.0000	1.000	1.000	1.0000	1.0000	1.0000	1.000	1.0000
11	0.9932	0.990	0.993	0.9931	0.993	0.993	0.9928	0.9929	0.9931	0.993	0.9929
12	0.9865	0.985	0.986	0.9864	0.987	0.986	0.9866	0.9859	0.9861	0.986	0.9859
13	0.9799	0.977	0.979	0.9797	0.980	0.979	0.9800	0.9790	0.9792	0.979	0.9790
14	0.9733	0.970	0.973	0.9731	0.974	0.973	0.9729	0.9722	0.9724	0.972	0.9722
15	0.9669	0.963	0.967	0.9666	0.967	0.966	0.9666	0.9655	0.9657	0.966	0.9655
16	0.9605	0.955	0.960	0.9603	0.961	0.960	0.9605	0.9589	0.9591	0.959	0.9589
17	0.9542	0.949	0.954	0.9539	0.954	0.953	0.9535	0.9524	0.9526	0.952	0.9523
18	0.9480	0.942	0.948	0.9477	0.948	0.947	0.9470	0.9460	0.9462	0.946	0.9459
19	0.9420	0.935	0.942	0.9416	0.941	0.941	0.9417	0.9396	0.9399	0.940	0.9395
20	0.9359	0.928	0.935	0.9355	0.935	0.935	0.9343	0.9333	0.9336	0.933	0.9333
21	0.9300	0.922	0.929	0.9295	0.929	0.929	0.9283	0.9272	0.9274	0.927	0.9271
22	0.9241	0.915	0.924	0.9236	0.923	0.923	0.9221	0.9211	0.9212	0.921	0.9210
23	0.9183	0.909	0.918	0.9177	0.917	0.917	0.9178	0.9151	0.9151	0.915	0.9150
24	0.9125	0.903	0.912	0.9120	0.911	0.911	0.9112	0.9091	0.9091	0.909	0.9090
25	0.9068	0.897	0.906	0.9063	0.906	0.905	0.9067	0.9033	0.9032	0.903	0.9032
26	0.9012	0.892	0.901	0.9007	0.900	0.900	0.8997	0.8975	0.8974	0.898	0.8974
27	0.8957	0.886	0.895	0.8951	0.895	0.894	0.8949	0.8918	0.8917	0.892	0.8917
28	0.8902	0.880	0.889	0.8896	0.889	0.889	0.8900	0.8861	0.8860	0.886	0.8860
29	0.8848	0.874	0.884	0.8842	0.884	0.883	0.8824	0.8806	0.8804	0.881	0.8805
30	0.8795	0.867	0.879	0.8788	0.878	0.878	0.8773	0.8751	0.8748	0.875	0.8750
31	0.8742	0.861	0.873	0.8735	0.873	0.872	0.8720	0.8696	0.8693	0.870	0.8695
32	0.8690	0.856	0.868	0.8683	0.868	0.867	0.8664	0.8643	0.8638	0.864	0.8641
33	0.8639	0.852	0.863	0.8632	0.863	0.862	0.8611	0.8590	0.8584	0.859	0.8588
34	0.8588	0.847	0.858	0.8580	0.858	0.857	0.8583	0.8537	0.8531	0.854	0.8536
35	0.8538	0.842	0.853	0.8530	0.852	0.852	0.8526	0.8486	0.8479	0.849	0.8484
36	0.8488	0.837	0.848	0.8480	0.847	0.847	0.8466	0.8435	0.8428	0.844	0.8433
37	0.8439	0.832	0.843	0.8431	0.842	0.842	0.8436	0.8384	0.8378	0.838	0.8383
38	0.8391	0.827	0.838	0.8382	0.837	0.837	0.8373	0.8334	0.8329	0.833	0.8333
39	0.8343	0.822	0.833	0.8334	0.832	0.832	0.8306	0.8285	0.8281	0.829	0.8284
40	0.8295	0.817	0.829	0.8287	0.828	0.827	0.8272	0.8236	0.8233	0.824	0.8235
41	0.8249	0.814	0.824	0.8239	0.823	0.823	0.8237	0.8188	0.8186	0.819	0.8187
42	0.8202	0.811	0.819	0.8193	0.819	0.818	0.8164	0.8141	0.8139	0.814	0.8139
43	0.8156	0.808	0.815	0.8147	0.814	0.813	0.8125	0.8094	0.8093	0.809	0.8092
44	0.8111	0.805	0.810	0.8102	0.810	0.809	0.8084	0.8017	0.8047	0.802	0.8045
45	0.8066	0.802	0.806	0.8057	0.805	0.804	0.8041	0.8001	0.8001	0.800	0.8000
46	0.8022	0.799	0.801	0.8013	0.800	0.800	0.7995	0.7956	0.7956	0.796	0.7954
47	0.7978	0.797	0.797	0.7969	0.796	0.795	0.7946	0.7911	0.7911	0.791	0.7909
48	0.7935	0.795	0.792	0.7925	0.792	0.791	.....	0.7866	0.7866	0.787	0.7865
49	0.7892	0.793	0.788	0.7882	0.787	0.787	.....	0.7823	0.7821	0.782	0.7821
50	0.7849	0.791	0.784	0.7839	0.782	0.783	.....	0.7779	0.7777	0.778	0.7777
51	0.7807	...	0.781	0.7797	...	0.778	.....	.....	0.7733	0.773	0.7734
52	0.7766	...	0.776	0.7756	...	0.774	.....	.....	0.7689	0.769	0.7692
53	0.7725	...	0.771	0.7714	...	0.770	.....	.....	0.7646	0.765	0.7650
54	0.7684	...	0.769	0.7674	...	0.766	.....	.....	0.7603	0.760	0.7608
55	0.7644	...	0.763	0.7633	...	0.762	.....	.....	0.7560	0.756	0.7567
56	0.7604	...	0.759	0.7593	...	0.758	.....	.....	0.7518	0.752	0.7526
57	0.7565	...	0.755	0.7554	...	0.754	.....	.....	0.7476	0.748	0.7486
58	0.7526	...	0.751	0.7515	...	0.750	.....	.....	0.7435	0.744	0.7446
59	0.7487	...	0.748	0.7476	...	0.746	.....	.....	0.7394	0.739	0.7407
60	0.7449	...	0.744	0.7438	...	0.742	.....	.....	0.7354	0.735	0.7368
61	0.7411	...	0.740	0.7399	...	0.738	.....	.....	0.7314	...	0.7329
62	.....	...	0.736	0.7362	...	0.735	.....	.....	0.7275	...	0.7290
63	.....	...	.....	.....	...	0.731	.....	.....	.....	...	0.7253
64	.....	...	.....	.....	...	0.727	.....	.....	.....	...	0.7216
65	.....	...	.....	.....	...	0.724	.....	.....	.....	...	0.7179
66	.....	...	.....	.....	...	0.720	.....	.....	.....	...	0.7142
67	.....	...	.....	.....	...	0.716	.....	.....	.....	...	0.7106
68	.....	...	.....	.....	...	0.713	.....	.....	.....	...	0.7070
69	.....	...	.....	.....	...	0.709	.....	.....	.....	...	0.7035
70	.....	...	.....	.....	...	0.706	.....	.....	.....	...	0.7000
71	.....	...	.....	.....	...	0.702	.....	.....	.....	...	0.6965
72	.....	...	.....	.....	...	0.699	.....	.....	.....	...	0.6930
73	.....	...	.....	.....	...	0.696	.....	.....	.....	...	0.6896
74	.....	...	.....	.....	...	0.692	.....	.....	.....	...	0.6863
75	.....	...	.....	.....	...	0.689	.....	.....	.....	...	0.6829
76	.....	...	.....	.....	...	0.686	.....	.....	.....	...	0.6796
77	.....	...	.....	.....	...	0.682	.....	.....	.....	...	0.6763

[1] Bache & McCulloh, 1848; Watt's Dict., 1865, vol. III; U. S. Petroleum Ass'n, 1864; Handwörterbuch der Chemie, 1859, vol. II, 1; Dingler's Poly. Journal, 1870, vol. 198; Tucker, Manual of Sugar Analysis, 1881; Johnson's Cycl., Vol. II, 1876; Fownes's Chemistry, 12th ed., 1877; Ure's Dict., vol. I, 1867; Neues Handwörterbuch, 1871, Vol. I; Deut. Chem. Kalender, Dresden, 1877. [2] Trans. Philos., 1794; Annales de Chimie, 1797; Children, 1819; Bache & McCulloh, 1848. [3] Chemiker Kalender, Berlin, 1881, 1882. [4] Hoffmann-Schaedler, Tabellen, 1877; Dingler's Poly. Journal, Vol. XXVII, 1828. [5] Bache & McCulloh, 1848; Pharmacopœa Batava, 1805. [6] U. S. Dispensatory, 11th, 12th, 13th, 14th eds.; Neues Handwörterbuch, 1871, Vol. I. [7] Bolley Handbuch der Chem. Technologie, 1865. [8] Bache & McCulloh, 1848; Handwörterbuch der Chemie, Vol. II, 1, 1859. [9] Duncan's Edinburgh Disp., 1830; U. S. Dispensatory, 5th, 7th, 8th, 11th, 12th, 13th, 14th eds. [10] Zimrek, Technologische, Tabellen & Notizen, 1863. [11] Philadelphia Coll. Pharmacy; U. S. Disp., 12th, 13th, 14th eds.; Mott, Chemist's Manual, 1877.

NOTE.—The modulus for each scale was calculated by the formula, $n = \frac{P(d-10)}{1-P}$, in which n = modulus, d = Baumé degree; P = specific gravity.

The calculations were in each case made on the 47th Baumé degree.

TABLE III.—*Specific gravity of a 15 per cent. solution of common salt at* 10° *R.* (=12.5° *C.*=54.5° *F.*).

No.	Specific gravity.	By whom calculated.
1	1.1122	Chandler and Wiechmann.
2	1.1121	do.
3	1.1120	do.
4	1.1122	do.
5	1.1126	do.
6	1.1121	Prof. Henry Morton.
7	1.1119	Dr. Hermann Endemann.
8	1.1110	Dr. Arno Behr.
9	1.1110	M. Baudin (Chem. News, 1870, XXI, 54).
10	1.110725	Prof. Coulier, *Ibid.*
11	1.11146	Dr. Gerlach (Zeit. Anal. Chemie, 1865, IV, 1).
12	1.1160	E. Soubeiran (Traité de Pharmacie 3ème ed., 1847, I, 13).
13	1.1094	Francœur (Mémoire sur l'Aréométrie, Paris, 1842, 26).
	1.111898	Average.

NOTE.—1 and 2 were chemically pure salt; 3 and 4 were Syracuse solar salt; 5 was Syracuse factory-filled dairy salt.

It should be remarked that in the above table the number by Baudin was obtained by weighing the solution at 15° centigrade instead of 12.5, and Gerlach's result was obtained by weighing at 14° centigrade, and *calculating* what the specific gravity would be at 15° centigrade *in vacuo*. Francœur determined his specific gravity at the maximum density of water.

None of these determinations were rejected, however, in making up the table, as the numbers are so nearly alike. We may fairly assume that the average is practically 1.1119.

Table IV exhibits a scale which has been carefully calculated by Mr. Wiechmann from the actual average as given on Table III, by the formulæ

$$n=\frac{P\times d}{P-1} \qquad P=\frac{n}{n-d}$$

In which P=the specific gravity; d=the Baume degree; n=the modulus.

TABLE IV.—*Value of degrees Baumé calculated from 0°=1, and 15°=1.1118988 by the modulus 149.04969, the experimental work having been conducted in exact accordance with Baumé's original directions.*

[Temperature 10° R.=12.5° C.=54.5° F.]

Baume degrees.	Specific gravity.	Baume degrees.	Specific gravity.	Baume degrees.	Specific gravity.	Baumé degrees.	Specific gravity.
0	1.00000	20	1.15497	39	1.35438	58	1.63701
1	1.00675	21	1.16399	40	1.36680	59	1.65519
2	1.01360	22	1.17316	41	1.37945	60	1.67378
3	1.02054	23	1.18246	42	1.39234	61	1.69279
4	1.02757	24	1.19192	43	1.40547	62	1.71223
5	1.03471	25	1.20153	44	1.41885	63	1.73213
6	1.04191	26	1.21129	45	1.43248	64	1.75250
7	1.04927	27	1.22122	46	1.44638	65	1.77335
8	1.05671	28	1.23131	47	1.46056	66	1.79470
9	1.06426	29	1.24156	48	1.47501	67	1.81657
10	1.07191	30	1.25199	49	1.48975	68	1.83899
11	1.07968	31	1.26260	50	1.50479	69	1.86196
12	1.08755	32	1.27338	51	1.52014	70	1.88551
13	1.09555	33	1.28436	52	1.53580	71	1.90967
14	1.10366	34	1.29552	53	1.55179	72	1.93446
15	1.11189	35	1.30688	54	1.56812	73	1.95989
16	1.12025	36	1.31844	55	1.58479	74	1.98601
17	1.12873	37	1.33021	56	1.60182	75	2.01283
18	1.13735	38	1.34218	57	1.61923	76	2.04038
19	1.14609						

It will be seen by comparing Table IV with Table I that this scale corresponds most closely with No. 20, which is entitled "Baumé's original scale," and which was calculated by Gerlach in 1870—Dingler's Pol. J., vol. 198, 314—and was based upon the specific gravity 1.11146 for the 15 per cent. salt solution. The observation, however, was made at 14° centigrade and was then calculated for 15° centigrade in vacuo, while Baumé's directions are to use a 15 per cent. salt solution at 10° Réaumur in the atmosphere. It will be seen that neither this nor any other of the twenty-three scales published in Table I has been obtained by strictly following Baumé's directions.

SCALES FOR LIQUIDS LIGHTER THAN WATER.

For the purpose of ascertaining the exact value of Baumé's degrees for liquids lighter than water, three 10 per cent. salt solutions were carefully prepared, using as before chemically pure salt, "Solar" Syracuse salt, and Syracuse "Factory-filled dairy salt." The results are exhibited in Table V, together with results obtained by other chemists.

TABLE V.

Specific gravity of a 10 per cent. solution of common salt at 10° R. (=12.5° C. =54.5° F.)

1.	1.0738	Chandler and Wiechmann.
2.	1.0737	Chandler and Wiechmann.
3.	1.0741	Chandler and Wiechmann.
4.	1.07303	Schober and Pecher (Dingl. Pol. J., 1828, XXVII, 65).
5.	1.07518	Schober and Pecher.
6.	1.07372	Schober and Pecher.
7.	1.073464	Dr. Gerlach (Zeit. Anal. Chemie, 1865, IV, 8).
8.	1.073405	Dr. Gerlach (Zeit. Anal. Chemie, 1865, IV, 8).
9.	1.07350	Francœur (Mémoire sur l'Aréometrie, Paris, 1842, 26).
	1.0737665	Average.

NOTE.—1 was chemically pure salt; 2 was Syracuse solar salt; 3 was Syracuse factory-filled dairy salt; 4 was rock salt; 5 was chemically pure salt; 6 was commercial salt.

The average of these determinations gives as the specific gravity of a 10 per cent. salt solution 1.0737665, and the modulus is=145.56289, computed according to the formula

$$n=\frac{P\,(d-10)}{1-p}$$

in which P=the specific gravity, d=the Baumé degree, n=the modulus.

With the use of this modulus the following table (Table VI) has been calculated by the formula

$$P=\frac{n}{(n-10)+d}$$

in which P=the specific gravity, d=the Baumé degree, n=the modulus.

TABLE VI.—*Value of degrees Baumé calculated from 0°=1.0737665 and 10°=1 by the modulus 145.56289, the experimental work having been conducted in exact accordance with Baumé's original directions.*

[Temperature 10° R.=12.5° C=54.5° F.]

Baume degree.	Specific gravity.
10	1.00000
15	0.96679
20	0.93571
25	0.90657
30	0.87919
35	0.85342
40	0.82912
45	0.80616
50	0.78443
55	0.76385
60	0.74432
65	0.72577
70	0.70811
75	0.69130

On comparing Table VI with Table II it will be seen that it agrees most closely with the first scale, which is Francœur's, and which has been adopted by the United States Petroleum Association.

CONCLUSION.

In conclusion I would suggest to the Academy that, owing to the very extensive use which is made of the Baumé instruments, it would be eminently proper to consider the propriety of legis-

lation on the part of Congress, or some other means, for establishing a fixed value to the two scales of the Baumé instruments, and I will offer at the proper time the following resolution:

"*Resolved*, That a committee be appointed to consider what action, if any, is desirable, with a view to establishing a legal value for the degrees of the Baumé and other hydrometers of arbitrary scales; the committee to report at the next meeting."

NOTE.—This resolution was adopted, and the following committee was appointed: Julius E. Hilgard, Superintendent United States Coast Survey, Washington, D. C.; Henry Morton, President Stevens Institute, Hoboken, N. J.; C. F. Chandler, Professor of Chemistry, Columbia College, New York.

I would further state that I am very largely indebted to my assistant, F. G. Wiechmann, Ph.B., for the experimental and historical data contained in the preceding tables.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

FIFTH MEMOIR.

ON SMALL DIFFERENCES OF SENSATION.

ON SMALL DIFFERENCES OF SENSATION.

READ OCTOBER 17, 1884.

By C. S. Peirce and J. Jastrow.

The physiological psychologists assume that two nerve excitations alike in quality will only produce distinguishable sensations provided they differ in intensity by an amount greater than a fixed ratio. The least perceptible difference of the excitations divided by half their sum is what they call the *Unterschiedsschwelle*. Fechner* gives an experiment to prove the fact assumed, namely: He finds that two very dim lights placed nearly in line with the edge of an opaque body show but one shadow of the edge. It will be found, however, that this phenomenon is not a clearly marked one, unless the lights are nearly in range. If the experiment is performed with lateral shifting of one of the lights, and with a knowledge of the effects of a telescope upon the appearance of terrestrial objects at night, it will be found very far from conclusive.

The conception of the psychologists is certainly a difficult one to seize. According to their own doctrine, in which the observed facts seem fully to bear them out, the intensity of the sensation increases continuously with the excitation, so that the least increase of the latter must produce a corresponding increase of the former. And, indeed, the hypothesis that a continuous increase of the excitation would be accompanied by successive discrete increments of the sensation, gratuitous as it would be, would not be sufficient to account for a constant *Unterschiedsschwelle*. We are therefore forced to conclude that if there be such a phenomenon it has its origin, not in the faculty of sensation, but in that of comparing sensations. In short, if the phenomenon were established, we should be forced to say that there was a least perceptible difference of sensation—a difference which, though existing in sensation, could not be brought into consciousness by any effort of attention. But the errors of our judgments in comparing our sensations seem sufficiently accounted for by the slow and doubtless complicated process by which the impression is conveyed from the periphery to the brain; for this must be liable to more or less accidental derangement at every step of its progress. Accordingly we find that the frequencies of errors of different magnitudes follow the probability curve, which is the law of an effect brought about by the sum of an infinite number of infinitesimal causes. This theory, however, does not admit of an *Unterschiedsschwelle*. On the contrary, it leads to the method of least squares, according to which the multiplication of observations will indefinitely reduce the error of their mean, so that if of two excitations one were ever so little the more intense, in the long run it would be judged to be the more intense the majority of times. It is true that the astronomers themselves have not usually supposed that this would be the case, because (apart from constant errors, which have no relevancy to the present question) they have supposed this extreme result to be contrary to common sense. But it has seemed to us that the most satisfactory course would be to subject the question to the test of direct experiment. If there be a least perceptible difference, then when two excitations differing by less than this are presented to us, and we are asked to judge which is the greater, we ought to answer wrong as often as right in the long run. Whereas, if the theory of least squares is correct, we not

* Elemente der Psychophysik, I, p. 242.

only ought to answer right oftener than wrong, but we ought to do so in a predictible ratio of cases.*

We have experimented with the pressure sense, observing the proportion of errors among judgments as to which is the greater of two pressures, when it is known that the two are two stated pressures, and the question presented for the decision of the observer is, which is which? From the probability, thus ascertained, of committing an error of a given magnitude, the probable error of a judgment can be calculated according to the mathematical theory of errors. If, now, we find that when the ratio of the two pressures is smaller than a certain ratio, the erroneous judgments number one-half of the whole, while the mathematical theory requires them to be sensibly fewer, then this theory is plainly disproved, and the maximum ratio at which this phenomenon is observed the so-called *Unterschiedsschwelle*. If, on the other hand, the values obtained for the probable error are the same for errors varying from three times to one-fourth of the probable error (the smallest for which it is easy to collect sufficient observations), then the theory of the method of least squares is shown to hold good within those limits, the presumption will be that it extends still further, and it is possible that it holds for the smallest differences of excitation. But, further, if this law is shown to hold good for difference so slight that the observer is not conscious of being able to discriminate between them at all, all reason for believing in an *Unterschiedsschwelle* is destroyed. The mathematical theory has the advantage of yielding conceptions of greater definiteness than that of the physiologists, and will thus tend to improve methods of observation. Moreover, it affords a ready method for determining the sensibility or fineness of perception and allows of a comparison with the results of others; for, knowing the number of errors in a certain number of experiments, and accepting the conclusions of this paper, the calculated ratio to the total excitation of that variation of excitation, in judging which we should err one time out of four, measures the sensibility. Incidentally our experiments will afford additional information upon the value of the normal average sensibility for the pressure sense, which they seem to make a finer sense than it has hitherto been believed to be. But in this regard two things have to be noted: (1) Our value relates to the probable error or the value for the point at which an error is committed half the time; (2) in our experiments there were two opportunities for judging, for the initial weight was either first increased and then diminished, or *vice versa*, the subject having to say which of these two double changes was made. It would seem at first blush that the value thus obtained ought to be multiplied by $\sqrt{2}$ (1.414) to get the error of a single judgment. Yet this would hardly be correct, because the judgment, in point of fact, depended almost exclusively on the sensation of increase of pressure, the decrease being felt very much less. The ratio $\sqrt{2}$ (1.414) would therefore be too great, and 1.2 would perhaps be about correct. The advantage of having two changes in one experiment consists in this: If only one change were employed, then some of the experiments would have an increase of excitation only and the others a decrease only; and since the former would yield a far greater amount of sensation than the latter, the nature of the results would be greatly complicated; but when each experiment embraces a

* The rule for finding this ratio is as follows: Divide the logarithm of the ratio of excitations by the probable error and multiply the quotient by 0.477. Call this product t. Enter it in the table of the integral θt, given in most works on probabilities; θt is the proportion of cases in which the error will be less than the difference between the given excitations. In all these cases, of course, we shall answer correctly, and also by chance in one-half of the remaining cases. The proportion of erroneous answers is therefore $(1-\theta t)\div 2$. In the following table the first column gives the quotient of the logarithm of the ratio of excitation, divided by the probable error, and the second column shows the proportion of erroneous judgments:

0.0	0.50
0.05	0.49
0.1	0.47
0.25	0.43
0.5	0.37
1.0	0.25

To guess the correct card out of a pack of fifty-two once in eleven times it would be necessary to have a sensation amounting to 0.37 of the probable error. This would be a sensation of which we should probably never become aware, as will appear below.

double change this difference in the amount of sensation caused by an increase and decrease of pressure affects every experiment alike, and the liability to error is constant.*

Throughout our observations we noted the degree of confidence with which the observer gave his judgment upon a scale of four degrees, as follows:

0 denoted absence of any preference for one answer over its opposite, so that it seemed non sensical to answer at all.

1 denoted a distinct leaning to one alternative.

2 denoted some little confidence of being right.

3 denoted as strong a confidence as one would have about such sensations.

We do not mean to say that when zero was the recorded confidence, there was absolutely no sensation of preference for the answer given. We only mean that there was no sensation that the observer noticed when attending to his feelings of this sort as closely as he conveniently could, namely, closely enough to mark them on this scale. The scale of confidence fluctuated considerably. Thus, when Mr. Jastrow passed from experiments upon differences of weight of 60, 30, and 15 on the thousand to differences of 20, 10, and 5 on the thousand, although the accuracy of his judgments was decidedly improved, his confidence fell off very greatly, owing to his no longer having the sensation produced by a difference of 60 present to his memory. The estimations of confidence were also rough, and might be improved in future work. The average marks seem to conform to the formula—

$$m = c \log \frac{p}{1-p}$$

where m denotes the degree of confidence on the scale, p denotes the probability of the answer being right, and c is a constant which may be called the index of confidence.

To show that this formula approximates to the truth, we compare it with the average marks assigned to estimates of differences for which more than a hundred experiments were made. Mr. Jastrow's experiments are separated into groups, which will be explained below.

First group.

Ratio of pressures.	Peirce, observer.		Jastrow, observer.			
	$c=1.25$.		$c=1.5$.		$c=0.6$.	
	Mean confidence.		Mean confidence.		Mean confidence.	
	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
1.015	0.14	0.10	0.30	0.2	0.34	0.2
1.030	0.30	0.35	0.40	0.42	0.55	0.76
1.060	0.70	0.70	0.85	0.87	1.02	1.32

Ratio of pressures.	Jastrow, observer.			
	$c=0.25$.		$c=0.4$.	
	Mean confidence.		Mean confidence.	
	Observed.	Calculated.	Observed.	Calculated.
1.005	0.00	0.03	0.00	0.06
1.010	0.07	0.06	0.05	0.12
1.020	0.12	0.12	0.50	0.39

* The number of errors, when an increase of weight was followed by a decrease, was slightly less than when the first change was a decrease of pressure.

The judgments enunciated with any given degree of confidence were more likely to be right with greater differences than with smaller differences. To show this, we give the frequency of the different marks in Mr. Jastrow's second, third, and fourth groups.*

The apparatus used was an adaptation of a "Fairbanks" post-office scale; upon the end of the beam of which was fixed a square enlargement (about one-half inch square), with a flat top, which served to convey the pressure to the finger in a manner to be presently described. This was tightly covered with an India-rubber cap, to prevent sensations of cold, &c., from contact with the metal. A kilogram placed in the pan of the balance brought a pressure of one fourth

* The result of our observations on the confidence connected with the judgments is as follows:

[Subject, Mr. Peirce.]

Variations.	Average confidence.	Number of sets of 50.
Grams.		
60	.67	7
30	.28	6
15	.15	5

[Subject, Mr. Jastrow.]

Variations.	Average confidence.	Number of sets of 50.
60	.90	13
30	.51	12
15	.30	12
20	.11	12
10	.06	12
5	.00	10

In 1,125 experiments (subject, Mr. Peirce)—variations 15, 30, and 60 grams—there occurred confidence of 3, 35 times (3 per cent.): of 2, 102 times (9 per cent.): of 1, 282 times (25 per cent.): of 0, 706 times (63 per cent.). In these experiments there were 332 (29 per cent.) errors committed, of which 1 (0.3 per cent.) was made in connection with a confidence 3; 10 (3 per cent.) with a confidence 2; 51 (15 per cent.) with a confidence 1; 270 (81 per cent.) with a confidence 0. From which we find that in connection with a confidence of 3 there occurred 1 error in 35 cases (3 per cent.); with a confidence of 2, 10 errors in 102 cases (10 per cent.); with a confidence of 1, 51 errors in 282 cases (18 per cent.); with a confidence of 0, 270 errors in 706 cases (38 per cent.).

In 1,975 experiments (subject, Mr. Jastrow)—variations 15, 30, and 60 grams—there occurred confidence of 3, 62 times (3 per cent.): of 2, 196 times (10 per cent.); of 1, 594 times (30 per cent.); of 0, 1,123 times (57 per cent.). In these experiments there were 451 (23 per cent.) errors committed, of which 2 (0.4 per cent.) were made in connection with a confidence of 3: 12 (3 per cent.) with a confidence of 2; 97 (22 per cent.) with a confidence of 1, 340 (75 per cent.) with a confidence of 0. Again, in connection with a confidence of 3, errors occurred twice in 62 cases (3 per cent.); with a confidence of 2, 12 times in 196 cases (6 per cent.); with a confidence of 1, 97 times in 504 cases (16 per cent.); with a confidence of 0, 340 times in 1,123 cases (30 per cent.).

In 1,675 experiments (subject, Mr. Jastrow)—variations 5, 10, and 20 grams—there occurred confidences of 3, none; of 2, none; of 1, 115 times (7 per cent.); of 0, 1,560 times (93 per cent.). In these experiments there were 538 (32 per cent.) errors committed, of which 16 (3 per cent.) occurred in connection with a confidence of 1; 522 (97 per cent.) with a confidence of 0. Again, in connection with a confidence of 1, errors occurred 16 times in 115 cases (14 per cent.); with a confidence of 0, 522 times in 1,560 cases (34 per cent.).

Second group.

Ratio of weights.	Mark 0.	Mark 1.	Mark 2.	Mark 3.
1.015	110 right 66 wrong	51 right 17 wrong	3 right 2 wrong	1 right 0 wrong
1.030	106 right 35 wrong	72 right, 11 wrong	23 right 1 wrong	2 right 0 wrong
1.060	86 right 8 wrong	75 right, 1 wrong	54 right 2 wrong	21 right 0 wrong

of its weight upon the finger. The differential pressure was produced by lowering upon the pan of the balance a smaller pan into which the proper weights could be firmly fixed; this little pan had its bottom of cork, and was placed upon a piece of flannel which constantly remained in the pan of the balance. It was lifted off and on by means of a fine India-rubber thread, which was so much stretched by the weight as certainly to avoid any noise or jar from the momentum of the descending pan. A sufficient weight could also be hung on the beam of the balance, so as to take off the entire pressure from the finger at the end of each experiment. This weight could be applied or removed by means of a cam acting upon a lever; and its bearings upon the beam were guarded by India-rubber. It was found that the use of this arrangement, which removed all annoying irregularities of sensation connected with the removal and replacement of the greater (initial) pressure, rendered the results more uniform and diminished the probable error. It also shortened the time necessary for performing the experiments, so that a series of 25 experiments was concluded before the effects of fatigue were noticeable. It may be mentioned that certain causes tended to the constant decrease of the probable error as the experiments went on, these mainly being an increased skill on the part of the *operator* and an education of the sensibility of the *subject*. The finger was supported in such a way as to be lightly but firmly held in position, all the muscles of the arm being relaxed; and the India-rubber top of the brass enlargement at the end of the beam of the balance was never actually separated from the finger. The projecting arm of a filter-stand (the height of which could be adjusted) with some attachments not necessary to detail, gently prevented the finger from moving upwards under the pressure exerted by the weight in the pan. In the case of Mr. Peirce as subject (it may be noted that Mr. Peirce is left-handed, while Mr. Jastrow is strongly right-handed) the tip of forefinger, and in the case of Mr. Jastrow of the middle finger, of the left hand were used. In addition, a screen served to prevent the subject from having any indications whatever of the movements of the operator. It is hardly necessary to say that we were fully on our guard against unconsciously received indications.

The observations were conducted in the following manner: At each sitting three differential weights were employed. At first we always began and ended with the heaviest, but at a later period the plan was to begin on alternate days with the lightest and heaviest. When we began with the heaviest 25 observations* were made with that; then 25 with the middle one, and then 25 with the lightest; this constituted one-half of the sitting. It was completed by three more sets of 25, the order of the weights being reversed. When we began with the lightest the heaviest was used for the third and fourth sets. In this way 150 experiments on each of us were taken at one sitting of two hours.

A pack of 25 cards were taken, 12 red and 13 black, or *vice versa*, so that in the 50 experiments made at one sitting with a given differential weight, 25 red and 25 black cards should be used. These cards were cut exactly square and their corners were distinguished by holes punched in them so as to indicate the scale of numbers (0, 1, 2, 3) used to designate the degree of confidence of the judgment. The backs of these cards were distinguished from their faces. They were, in fact, made of ordinary playing-cards. At the beginning of a set of 25, the pack was well shuffled, and, the operator and subject having taken their places, the operator was governed by the color

Third and fourth groups.

[Marks 2 and 3 do not occur.]

Ratio of weights.	Mark 0.	Mark 1.
1.005	291 right 203 wrong	2 right 1 wrong
1.010	366 right 192 wrong	32 right 30 wrong
1.020	395 right 131 wrong	68 right 6 wrong

* At first a short pause was made in the set of 25, at the option of the subject; later this was dispensed with.

of the successive cards in choosing whether he should first diminish the weight and then increase it, or *vice versa*. If the weight was to be first increased and then diminished the operator brought the pressure exerted by the kilogram alone upon the finger of the subject by means of the lever and cam mentioned above, and when the subject said "change" he gently lowered the differential weight, resting in the small pan, upon the pan of the balance. The subject, having appreciated the sensation, again said "change," whereupon the operator removed the differential weight. If, on the other hand, the color of the card directed the weight to be first diminished and then increased, the operator had the differential weight already on the pan of the balance before the pressure was brought to bear on the finger, and made the reverse changes at the command of the subject. The subject then stated his judgment and also his degree of confidence, whereupon the total pressure was at once removed by the cam, and the card that had been used to direct the change was placed face down or face up according as the answer was right or wrong, and with corner indicating the degree of confidence in a determinate position. By means of these trifling devices the important object of rapidity was secured, and any possible psychological guessing of what change the operator was likely to select was avoided. A slight disadvantage in this mode of proceeding arises from the long runs of one particular kind of change, which would occasionally be produced by chance and would tend to confuse the mind of the subject. But it seems clear that this disadvantage was less than that which would have been occasioned by his knowing that there would be no such long runs if any means had been taken to prevent them. At the end of each set the results were of course entered into a book.*

The following tables show the results of the observations for each day:

Date.	Ratios of pressures. [Subject: Mr. Peirce.]						
	1.100	1.080	1.060	1.050	1.040	1.030	1.015
December 10	2 errors.	....	....	13 errors.	....	....	....
December 13	....	4 errors.	8 errors.	....	15 errors.	....	....
December 17	....	....	11	....	....	20 errors.	....
December 20	....	....	7	....	....	16	21 errors.
January 3	....	....	11	....	....	20	28
January 15	....	....	15	....	....	29	28
January 22	....	....	12	....	....	16	20
January 24	....	....	6	....	....	15	22
Means	2	4	10.4 ± 1.0	13	15	19.3 ± 1.4	21.6 ± 1.1
Calculated from probable error = 0.051	1.6 ± 1.0	7.2 ± 1.6	10.7 ± 0.8	12.7 ± 2.1	14.9 ± 2.2	17.2 ± 0.9	21.0 ± 1.1
Average confidence.							
Observed	1.9	0.9	0.7	0.8	0.3	0.3	0.2
Calculated	1.3	1.0	0.7	0.6	0.5	0.3	0.2

The numbers in the columns show the number of errors in fifty experiments. With the average number of errors in a set of fifty we compare the theoretical value of this average as calculated by the method of least squares. The number .051 thus obtained in this case best satisfies the mean number of errors. The numbers affixed with a sign denote, in the upper row the observed (*a posteriori*) probable error of the mean value as given, in the lower row the calculated (*a priori*) probable error. The last two lines give the average confidence observed and calculated with each variation of the ratios of pressure. It will be seen that the correspondence between the real and theoretical numbers is close, and closest when the number of sets is large. The probable errors also closely correspond, the observed being, as is natural, slightly larger than the calculated probable errors.

* In the experiments of December, 1883, and January, 1884, the method as above described was not fully perfected the most important fault being that the total weight instead of being removed and replaced by a mechanical device, was taken off by the operator pressing with his finger upon the beam of the balance.

The following is a similar table for Mr. Jastrow as subject:

Date.	Ratios of pressures. 1.100	1.080	1.060	1.050	1.040	1.030	1.020	1.015	1.010	1.005
December 10	5			19						
December 13		9	15		15					
December 17			11			23				
December 20			10			17		25		
January 3			8			14		24		
January 10			7			13		17		
January 15			12			6		22		
January 22			11			10		16		
January 24			4			11		18		
February 11			1			7		18		
February 17			2			10		17		
February 18			2			11		17		
February 24			2			8		15		
March 4							13		16	
March 5							13		17	
March 18							14		19	25
March 19							11		21	18
March 23							14		17	18
March 25							12		16	18
March 30							11		16	21
March 31							10		15	21
April 2							11		17	21
April 3							9		18	20
April 6							12		15	21
April 7			0			5	7	14	15	17
Means	5	9	6.6	19	15.0	11.6	11.4	18.9	16.8	20.5

It would obviously be unfair to compare these numbers with any set of theoretical numbers, since the probable error is on the decrease throughout, owing to effects of practice, etc. For various reasons we can conveniently group these experiments into four groups. The first will include the experiments from December 10 to January 22, inclusive; the second from January 24 to February 24, inclusive; the third from March 4 to March 25, inclusive; the fourth from March 30 to the end of the work.

The mean results for the different groups are exhibited in the following tables:

First group.

[Probable error=0.05.]

Ratios of pressures.	Number of sets of 50.	Average number of errors. Observed.	Average number of errors. Calculated from probable error.	Average confidence. Observed.	Average confidence. Calculated.
1.100	1	5	1.4 ± 1.4	0.9	1.5
1.080	1	9	7.0 ± 1.7	0.9	1.2
1.060	7	11.0 ± 0.7	10.4 ± 0.7	0.85	0.9
1.050	1	19	12.5 ± 2.1	0.35	0.7
1.040	1	15	14.7 ± 2.2	0.3	0.6
1.030	6	13.8 ± 1.5	17.0 ± 0.9	0.5	0.4
1.015	5	20.8 ± 1.1	21.0 ± 1.1	0.3	0.2

Second group.

[Probable error=0.0235.]

1.060	5	2.2 ± 0.3	2.1 ± 0.4	1.0	1.2
1.030	5	9.1 ± 0.6	9.6 ± 0.8	0.55	0.6
1.015	5	17.0 ± 0.3	16.6 ± 1.0	0.3	0.3

Third group.

[Probable error=0.02.]

Ratios of pressures.	Number of sets of 50.	Average number of errors.		Average confidence.	
		Observed.	Calculated from probable error.	Observed.	Calculated.
1.020	6	12.8±0.3	12.5±0.8	0.12	0.12
1.010	6	17.7±0.6	18.3±0.9	0.07	0.06
1.005	4	20.7±1.7	21.6±1.2	0.00	0.03

Fourth group.

[Probable error=0.0155.]

Ratios of pressures.	Number of sets of 50.	Average number of errors.		Average confidence.	
		Observed.	Calculated from probable error.	Observed.	Calculated.
1.060	1	0	0.8±0.6	1.6	....
1.030	1	5	4.8±1.4	0.5	0.4
1.020	6	10.0±0.5	9.6±0.8	0.1	0.2
1.015	1	14	12.8±2.1	0.1	0.13
1.010	6	16	16.5±0.9	0.05	0.12
1.005	6	20.8±0.4	20.6±1.0	0.00	0.06

The tables show that the numbers of errors follow, as far as we can conveniently trace them, the numbers assigned by the probability curve,* and therefore destroy all presumption in favor of an *Unterschiedsschwelle*. The introduction and retention of this false notion can only confuse thought, while the conception of the mathematician must exercise a favorable influence on psychological experimentation.†

The quantity which we have called the degree of confidence was probably the secondary sensation of a difference between the primary sensations compared. The evidence of our experiments

* In the tables of the third and fourth groups, there is a marked divergence between the *a priori* and *a posteriori* probable error, for the average number of errors in 50, making the observed probable error too small. This can only be partly accounted for by the fact that the subject formed the unconscious habit of retaining the number of each kind of experiment in a set and answering according to that knowledge. In point of fact the plus errors and minus errors separately do not exhibit the singular uniformity of their sums, for which we are quite unable to account. Thus in the fourth group we have:

Number of + and − errors.

Date.	1.020	1.010	1.005
March 30	−4, + 7	−6, +10	−13, + 8
March 31	7, + 3	−5, +10	− 6, +15
April 2	1, +10	−8, + 9	− 8, +13
April 3	−4, + 5	−4, +11	−10, +10
April 6	6, + 6	8, + 7	−10, +11
April 7	−5, + 9	−8, + 7	− 8, + 9

† The conclusions of this paper are strengthened by the results of a series of experiments on the color sense, made with the use of a photometer by Mr. Jastrow. The object was to determine the number of errors of a given magnitude, and compare the numbers thus ascertained with the theoretical numbers given by the probability curve. A thousand experiments were made. Dividing the magnitude of the errors from 0 to the largest error, made into 5 parts, the number of errors, as observed and calculated, that occur in each part are as follows:

Observed	199	181	217	213	190
Calculated	213	197	209	181	200

These numbers would be in closer accordance if the probable error were the same throughout, as it is not owing to the effects of practice, &c. Moreover, the experiments were made on different colors—300 on white and 100 each on yellow, blue, dove, pink, green, orange, and brown. These experiments were not continuous.

seems clearly to be that this sensation has no *Schwelle*, and vanishes only when the difference to which it refers vanishes. At the same time we found the subject often overlooked this element of his field of sensation, although his attention was directed with a certain strength toward it, so that he marked his confidence as *zero*. This happened in cases where the judgments were so much affected by the difference of pressures as to be correct three times out of five. The general fact has highly important practical bearings, since it gives new reason for believing that we gather what is passing in one another's minds in large measure from sensations so faint that we are not fairly aware of having them, and can give no account of how we reach our conclusions about such matters. The insight of females as well as certain "telepathic" phenomena may be explained in this way. Such faint sensations ought to be fully studied by the psychologist and assiduously cultivated by every man.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

SIXTH MEMOIR.

DESCRIPTION OF AN ARTICULATE OF DOUBTFUL RELATIONSHIP FROM THE TERTIARY BEDS OF FLORISSANT, COLO.

85

DESCRIPTION OF AN ARTICULATE OF DOUBTFUL RELATIONSHIP FROM THE TERTIARY BEDS OF FLORISSANT, COLORADO.

READ AT WASHINGTON, APRIL 20, 1882.

BY SAMUEL H. SCUDDER.

Among the remains of animals in my hands found in the ancient lake basin of Florissant are about forty specimens of an onisciform arthropod, about a centimeter in length, whose affinities have proved very perplexing. This does not result from poorness of preservation, for among the numerous specimens apparently all the prominent external features are found completely preserved, and even the course of some of the internal organs may occasionally be traced; but it presents such anomalies of structure that we are at a loss where to look for its nearest kin.

It appears to be an aquatic animal. Its body consists of three large subequal thoracic joints, and an abdomen about half as large again as any one of them, with occasional indications of a feeble division into four segments. These are the only jointed divisions that can be found in the body, there being no distinct head. The thoracic segments are so considered because each bears a pair of legs, which occur nowhere else. Their dorsal plates are large, flat longitudinally, and arched transversely; smooth, and deeply and narrowly notched in the middle of the front margin.

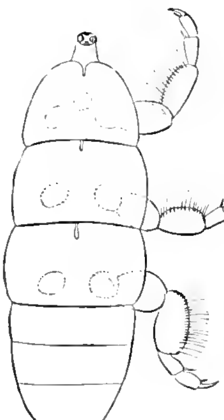


Fig. 1.

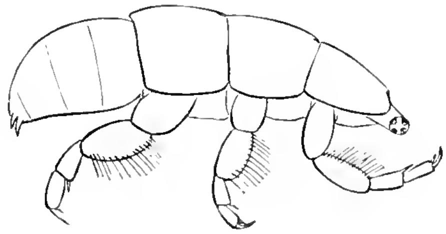


Fig. 2.

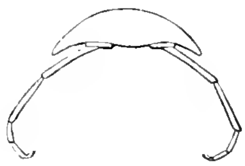


Fig. 3.

Fig. 1, dorsal view; fig. 2, lateral view; fig. 3, transverse sectional view of *Planocephalus aselloides* from the oligocene of Florissant, Colorado, restored, and magnified about six diameters.

The first plate, in which the median notch is more conspicuous and open than in the others, also narrows and becomes more arched in front, so as to form a sort of hood. The legs are very broad and compressed, and adapted to swimming, which was apparently their use, as there would be no need of such compression to crawl into chinks when the body is so much arched. They consist of a femur, tibia, and two tarsal joints, terminated by a single curved claw. The femur is very large, subovate, inserted (presumably by a coxa) in large cavities, those of opposite sides separated by their own width, and situated a little behind the middle of each segment. The tibia is also very large and subovate, but more elongated and squarer at the ends, being about twice as long as broad, and fringed on the anterior edge by a row of delicate hairs as long as the width of the joint. Of the two tarsal joints, the basal is a little the larger, being both longer and stouter. Each is armed at the tip internally with a tolerably stout spine of moderate length, and together they are a little longer than the tibia, much slenderer, and quadrate in form. The terminal claw

is about half as long as the terminal joint. The hind legs are somewhat stouter and the middle pair a little shorter than the others; but otherwise they closely resemble each other.

The different segments of the thorax, as stated, are protected above by the development of distinct chitinous plates, the lower edges of which are clearly marked, and extend downward to the concealment, on a side view, of the lower part of the body. The abdomen, however, seems to have no such specialization of the integument of the upper surface. It is stout, apparently well rounded transversely, and tapers to a produced but blunt tip, which is armed with a pair of slightly recurved stout claws, two or three times longer than the leg-claws, arranged as if to drag the body backward. The abdomen is faintly divided into four segments, often entirely obscured. Of these the terminal usually appears shorter than the others, which are subequal.

These divisions of the body are all that appear to have belonged to the animal; and it is the most remarkable fact in its organization that it certainly had no distinct chitinous head. This is the more surprising from the clearness with which the thoracic segments are marked. All that one can find preserved is what appears to be a ring of buccal plates terminating anteriorly the alimentary canal, and which was evidently capable of being thrust forward a long distance beyond the body. If it were not for the unusual preservation of the alimentary canal we should be forced to consider the head as lost from all the specimens, notwithstanding the nearly perfect preservation of the other parts; but in several specimens the alimentary tube can be traced with ease half through the body, terminating in front in these more or less clearly preserved chitinous plates, arranged to form a circle a little smaller than the coxal cavities. What is most remarkable is the extension of this alimentary tube and accompanying buccal plates like a proboscis far beyond the limits of the body; sometimes forward (apparently through the anterior notch) to a distance in front of the first segment equal to half the length of the latter: more often directed downward as well as outward, perhaps between the front legs, and occasionally extending beyond the body to nearly or quite *the entire length of the same.* It seems to leave its direct course within the body at about the middle of the first thoracic segment, directly in front of which position the buccal plates appear in one or two specimens, apparently in the position of repose. The various positions in which these buccal plates are found outside the body, both when their connection with the tube is traceable and when it is obscure or fails, shows how perfectly movable a proboscis the creature possessed. The external parts of the head, then, may be said to have probably been composed entirely of a flexible, extensible membrane capable of protrusion as a fleshy proboscis, separated by no line of demarkation from the first thoracic segment, and bearing as appendages only a series of buccal plates for mouth-parts, and beyond this nothing—neither cranium, eyes, antennæ, nor palpi. In the absence of eyes, one would naturally look for the development of tactile organs of some sort: but nothing of the kind is discoverable on the most careful special search, unless such an office may be performed by long delicate hairs which seem, in some few instances, to be scattered distantly over the projected mouth-tube.

A special study of the buccal plates in the twenty-four or twenty-five specimens which best show them gives no very satisfactory explanation of their form and relations. They have been said to form a ring, because in a considerable number they are so arranged; but it may be doubted whether this appearance is not due to the flaking of the chitinous parts. Like the lips of the notches of the thoracic segments, the buccal apparatus was evidently more dense and thicker than other tegumentary parts, for these are darker colored than the other parts and often carbonaceous In this condition the central portions seem liable to flake away and leave the thinner edges with ragged fragments of the carbonaceous inner portions attached, thus frequently forming a sort of irregular ring of dark chitine. On the other hand, it is just as common for fragments to become chipped out from the edges, or for rounded bits to fall out here and there, producing thereby an almost endless variety of present appearances. Among these it is difficult to trace the clew to the original arrangement and form of the plates. One might anticipate that these would have occurred around the central orifice of a proboscis; and if anything of this sort was present it would appear the most probable (though extremely doubtful) that there were four subtriangular plates of pretty large size, the lateral the larger, nearly meeting by their tips at the center. From specimens, however, which are least broken, it would seem quite as probable that the apparatus consisted of two attingent or overlapping circular plates, placed transversely, densest centrally, which by their

consolidation form an oval rounded mass. How such a pair of plates, or compound plates, could have subserved any purpose in the procuring of food, I cannot understand, but that such is their not unfrequent appearance, especially when seen through and protected by the thoracic shield of the first segment, is nevertheless the fact. It is to be hoped that other specimens may set this matter at rest. Those at hand allow no more definite statement than has been made. About three-fourths of the specimens of this species show the buccal plates more or less distinctly. In all but three they lie outside the body, usually at a distance from it of about half the length of the first thoracic segment. In a fourth specimen they lie half protruding at the front edge of the body.

These buccal plates, as already stated, are the only hard parts of the head, and the only appendages. Indeed, the only claim this portion of the body has to be called the head at all is that it is certainly the anterior extremity of the digestive canal. On account of this peculiarity of the organization of the head, the creature, which is certainly widely different from anything known, may be called *Planocephalus* (πλανάω, κεφαλή), and on account of its onisciform body, *Planocephalus aselloides*.

The first impression the sight of this strange headless creature conveys is that of an isopod crustacean. But the limited number of legs at once puts its reference to the Crustacea out of question, since no abdominal legs at all are present. Even in the parasitic Crustacea, where some of the legs are aborted, the same is the case with the segments themselves and with the joints of the legs which remain. The clear distinction which obtains between the thoracic and abdominal regions, and the limitation of the jointed legs to a single pair on each thoracic segment seems to lead one strongly to the conviction that these important elements of its construction place it among insects. The structure of the legs and the small tapering abdomen furnished with small anal appendages tend to the same conclusion.

Where among insects it should be placed is more questionable. Thinking it possibly a larval form, careful search has been made among all the groups into which it could by any possibility be presumed to fall, viz, among the Neuroptera and Coleoptera, but nothing in the slightest degree seeming to be related to it could be found, and its conspicuous size rendered it the less probable that a kindred form would be overlooked. On account, however, of its apterous character, and the discovery in recent years of certain curious types of animals (all of them, however, very minute) whose affinities have provoked more than usual discussion, my attention was early drawn toward certain resemblances which Planocephalus bears to the Pauropidæ among Myriapods and to the Thysanura, and here, if anywhere, its affinities seem likely to be found.

Its passing resemblance to the obtected forms of Pauropoda which Ryder has published under the name of Eurypauropodidæ is certainly very considerable, especially when it is remembered that the young of Pauropoda bear only three pairs of legs. The position of the more mobile part of the head of Eurypauropus beneath the cephalic shield is the same that the head of Planocephalus bears to the first thoracic shield; and the mouth-parts in both are confined to a somewhat similar circular area; there are no eyes in either, and the legs terminate in a single curved claw.

On the other hand, not only are antennæ of a highly organized character developed in Pauropoda, but the upper portion of the head carries a cephalic shield as large and conspicuous as the others; two pairs of legs are developed in the adult on every or nearly every segment of the body, and always on the abdominal to the same extent as on the thoracic segments, no abdomen being distinct from a thorax as in Planocephalus, but all the joints of the body entirely similar; the legs of the Pauropoda are formed on the myriapodal type, consisting of cylindrical undifferentiated joints, while those of Planocephalus are hexapodal in character, having a clearly defined femur and tibia, and a two-jointed tarsus conspicuously smaller and shorter than the preceding joints, of different form and apically spined.

The closer, therefore, we compare these two types the less important seem the points of resemblance, and the more important the points of divergence between them; for in the clear distinction of the thorax and abdomen, the absence of abdominal legs, and the structure of the legs themselves—fundamental features of its organization—Planocephalus clearly belongs to the true hexapod type of insects.

Its probable reference to the Thysanura may be defended on both negative and positive grounds. There is no other group of hexapods to which it could be considered as more likely to

belong, and there are some special thysanuran features in its structure, anomalous as it is. Since Packard has shown the reasonableness of placing the Symphyla (=Scolopendrella) of Ryder in the Thysanura, with the Collembola and Cinura as co-ordinate groups, the range of the Thysanura has been extended, and as a group of equivalent taxonomic value to the larger divisions of winged insects it has seemed itself to gain a better *ratio vivendi*. It is not necessary, therefore, in considering the relations of Planocephalus to Thysanura as a whole, to limit ourselves to points of comparison which it may have to one or another of its subordinate groups, but consider any points of resemblance we may find to any of these groups indifferently. The thoracic segments remind us not a little of some Cinura, while the abdomen as a whole recalls many of the Collembola, its approximated pair of specialized anal appendages being also like the variously developed organs of all Thysanura and unlike anything we can recall in any myriapod. The legs, in the development of the basal joints and in the smaller double-jointed tarsus, are closely related to those of some Cinura—built indeed upon the same general pattern, excepting that in Planocephalus they are specially developed for swimming. In the claw of our fossil genus we have something decidedly thysanuriform. We have heretofore spoken of the two tarsal joints as each armed apically with an interior spine; but that of the final joint arises from the base of the curving claw, and takes on more or less its direction, though only half as long as it, causing it to resemble very closely the smaller digit of the claw of both Collembola and Cinura, which is always inferior to the larger, and not infrequently, as in Lepidocyrtus, etc., straight instead of curved.

Of course, the rudimentary character of the head and the entire obliteration of the cephalic plates renders our fossil very distinct from any known type of Thysanura. But these features separate it quite as widely from any other group that may be suggested for it, and taking into account the considerable development of the thoracic portions, we must look upon Planocephalus as in some sense a degraded form, descended from a type in which the head was developed at least to some extent; and this renders it more probable that we have here found its proper place. Moreover when we examine the mouth-parts of Podura, we find them partially withdrawn within the head, reduced in external presentation to a small circle at the end of a conical protrusion of the under side of the head. Take away the cephalic plates, withdraw the mouth-parts to the same protection of the first thoracic segment which they now enjoy under the cephalic dome, imagine further that the mouth-parts could be protruded to their original position when covered by a cephalic shield, and we have about the same condition of things we find in Planocephalus; indeed the extensibility of the mouth parts beyond the thoracic shield seems quite what one might expect after the loss of the hard parts of the head; and the mouth-parts of Planocephalus bear much the same relative position to the first thoracic shield which those of Podura bear to the cephalic shield.

Assuming, then, that Planocephalus is a true hexapod, its general relations are certainly with the Thysanura rather than with any other group; while the character of the legs, the half developed double claw, and the anal appendages specialized to peculiar use are characters which are positively thysanuran. Add to this that we find in Podura something in a remote degree analogous to the extraordinary mouth-parts of Planocephalus, which we should in vain seek elsewhere, and the probability that we find here its nearest allies is rendered very strong; and the more so from the diversity of form and type in this group since the addition to it of Scolopendrella. The discovery of a co'lophore or something homologous to it would, we conceive, be decisive on the point; but the lateral preservation of nearly all the specimens of this fossil, and the obscurity of the base of the abdomen in nearly all, not only forbid its determination in those yet found, but render it doubtful if it will ever be discovered.

The position of this group among the Thysanura must be an independent one, between the Cinura and the Symphyla, and of an equivalent value to them. For such a group the name of BALLOSTOMA is proposed, in reference to the remarkable power it possessed of thrusting forward the gullet and mouth-parts. It would be characterized by the peculiarity named, by the lack of any chitinous frame-work of the head, the equal development of three thoracic segments developed dorsally as shields, and all separated from a cylindrical abdomen, which is armed at tip with a pair of hooks for crawling; legs largely developed and with expanded and flattened femora and tibiæ, the tarsi two jointed. The principal points toward which attention should be directed for the more perfect elucidation of its structure are the buccal plates and a possible collophore.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

SEVENTH MEMOIR.

THE STRUCTURE OF THE COLUMELLA AURIS IN THE PELYCOSAURIA.

THE STRUCTURE OF THE COLUMELLA AURIS IN THE PELYCOSAURIA

READ OCTOBER 16, 1884.

By E. D. COPE.

In a specimen of the Permian reptile *Clepsydrops leptocephalus* Cope,* the columella auris was found nearly in its normal position. It was found lying on the internal side of the normally joined squamosal and quadrate bones, the greater part of it within the former, but the distal extremity overlapping the superior part of the latter. These elements have lost their attachment to the cranium proper, so that the connection of the columella with the latter is not visible.

The columella is of unusual size as compared with other bones of the skull. Thus while the vertical length of the premaxillary bone is M. .060, and its width at the third tooth is .022, and while the vertical length of the quadrate bone is .085, the dimensions of the columella auris are as follows:

Length on inside of curve		.072
Greatest diameter just below stapes		.021
Distal diameters	long	.014
	short	.011
Diameters of head of epicolumella	long	.017
	short	.0095
Diameters of disk of stapes	long	.029
	short	.021

The shaft is slightly curved. The proximal extremity is divided by a fissure which is at right angles to the long transverse diameter. The smaller of these divisions is the more prominent, and its free extremital angle is formed by the continuous concave edge of the shaft. It bears the same relation to the shaft as the head of a rib does to its shaft (Fig. 1). The other proximal division occupies the position with reference to the shaft that the tubercle does to the rib. It is much larger than the inner head of the columella, and its face looks away from that of the head at an angle of 120°. Its long diameter diverges from that of the head by an angle of about 145°. Its free surface is a wide oval, and is concave, forming a basin-shaped lid to the foramen ovale of the internal ear. It thus represents the expanded proximal extremity of the stapes of other vertebrates. The base of this stapedial portion is perforated in the direction of its long diameter by a canal. One foramen of this canal is situated on the external edge below the external extremity of the oval basin. The other foramen issues in a groove, which continues for a short distance on the inner side of the bone from the fissure which separates the epicolumella from the stapes. This canal is, no doubt, that for the mandibular artery, and represents the foramen of the stapes, which is present in many Mammalia (Fig. 1 e e).

The distal extremity of the shaft is concave, and shows an articular surface of ridges and pits (Fig. 1 c). The coarseness of the latter indicates that the distal element attached at this point was cartilaginous, at least at the point of attachment. It will then resemble the corresponding part in the Crocodilia and Lacertilia, which connects the columella with the membranum tympani.

The points above determined as to the structure of this element permit of a number of interesting deductions.

First. This columella possesses what has not been previously observed in reptiles and higher

* Proceedings American Philosophical Society, 1881, p. 30.

vertebrates, an osseous connection, distinct from that formed by the stapes with the foramen ovale of the os petrosum. From this it follows that the stapes cannot be regarded as the proximal extremity of the visceral arch of which the columella forms a part, as its appearance in other reptiles would lead us to infer. It also lends support to the view of Salensky, which is accepted by Fraser, that the stapes is not an ossification of the cartilage of the visceral arch, but is an ossification of the tissue surrounding the mandibular artery.

Second. That the stapes resembles that of the Mammalia, and differs from that of other reptiles in the perforation below its head.

Third. That it is succeeded distally by a cartilaginous element, as in many other reptiles, which is the triangular ligament of Cuvier, and is functionally the analogue, and probably the homologue of the malleus of the Mammalia.

The homology of the proximal extremity of this columella may now be considered. It cannot be the suprastapedial cartilage of Huxley, since that is a superior process of the distal cartilaginous element or malleus. It appears to be unrepresented in the reptilian columella, and I have therefore called it the *epicolumella** (Figs. 1, Ecol).

In order to obtain some light on the homologies of the parts of this element, I have compared it with the corresponding parts in various species of reptiles and batrachians, several of which have been figured by Messrs. Huxley, Peters, and Parker. I have examined the ear bones and cartilages of the *Heloderma suspectum*, and append herewith the result of my observations:

The columella has the length usual in the Lacertilia, ceasing a short distance proximad to the eustachian foramen. The cartilage, which continues in the same straight line, is divided at the eustachian foramen, one process passing downwards on its anterior border, the other forming its superior border. The posterior branch continues downwards for a short distance and terminates in a point, which is connected by a short ligament with the extremity of the pterygoid bone (Fig. 2 hl). Immediately exterior to it, a slender, rod-like ligament descends in close contact with it. It extends farther, however, reaching the articular bone of the lower jaw immediately posterior to the cotylus for the quadrate (Fig. 2 el). Its subsequent course will be mentioned below. It appears to be the ligament which Peters has represented as continuous with the descending process of the stapedial cartilage, and on which he based his belief in the continuity of the latter with the cartilage of Meckel. Its superior connection is, however, not with any part of the ossicula auditus, but it can be traced to a point above the external extremity of the exoccipital bone.

The stapedial cartilage extends beyond the superior edge of the large eustachian foramen to the membranum tympani, and is there decurved, extending in contact with it for 2–3 mm. and terminating in an acute apex. Near the point where it reaches the membrane it sends a branch upwards and backwards (Fig. 2 sst,) the suprastapedial cartilage, which forms a slender rod. The suprastapedial reaches inwards, and terminates at a point on the inferior side of the exoccipital bone at a point a little within opposite the origin of the inferior branch. It is only connected with the horizontal cartilage below it by membrane, and it does not form a fan-shaped plate as represented by Peters in Stellio and Huxley in Hatteria.

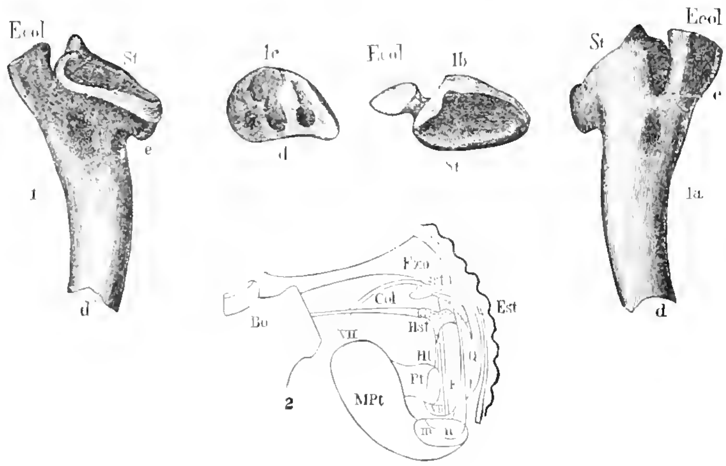
The following are the connections of the cartilages with adjacent elements: The distal extremity is acuminate and lies for a short distance on the membranum tympani, where it terminates without continuation. From the convexity of the curve formed by the inferior edge of the cartilage where it turns upwards, backwards, and inwards to form the suprastapedial, a narrow and weak band descends. It passes along the posterior border of the eustachian foramen, and terminates on the superior edge of the mandible. As it descends it thins out and becomes undistinguishable as a distinct rod or band. The slender rod already described as descending to the mandible from the descending process of the cartilage along the inner border of the eustachian foramen is figured by Peters in *Uromastix spinipes*.† He describes "it as a fibrous thread, which was formerly cartilaginous and connected the malleus with Meckel's cartilage." According to the figure it is not continuous with the inferior process of the cartilage ("malleus"). In *Heloderma suspectum* it passes anterior to the cartilage, in close contact with it, to a point superior to the suprastapedial process,

*American Naturalist, 1884, p. 1254.

†Monatsberichte Akademie Berlin, 1874, 44 f. B.

and then turns towards the base of the skull. I trace it directly to a foramen on the superior edge of the sphenoid. It is clearly the facial portion of the seventh nerve (tensor tympani), as described by Fischer and Stannius,* and has nothing to do with the auricular bones and cartilages. The only connection, then, with inferior arches which I can detect in this species is the fibrous one with the mandible, and I am doubtful of the significance of this.

It does not seem practicable to recognize the suprastapedial in the epicolumella of *Clepsydrops leptocephalus*.† It would require an excessive shortening of the columella, which might readily be the condition of things in Clepsydrops. But it would require that the suprastapedial should be ossified, and separated by suture from the remainder of the cartilage. Until some form is found in which this cartilage is segmented such a hypothesis has no foundation. The homology of the epicolumella with the incus is, on the other hand, almost certain; *first*, by the evident propriety of the exclusion of the stapes from the question, on account of its position, and by the history of its origin as shown by Salensky; *second*, on account of its position relative to both the stapes and the malleus. This being the case, the result follows that the doctrine of Peters that the quadrate bone is not the incus, as was maintained by Reichert, is the true one.‡



EXPLANATION OF PLATE.

FIG. 1. Columella auris of *Clepsydrops leptocephalus*: internal side. Fig. 1*a*, external side; 1*b*, proximal extremity; 1*c*, distal extremity; st., head of stapes; *Ecol.*, epicolumella: *d*, distal articular surface, especially represented in Fig. 1*c*: *e e*, foramina of stapedial canal. All figures are half natural size, excepting 1c, which is natural size.—From the proceedings of the American Philosophical Society, 1884, p. 46.

FIG. 2. Auricular bones and cartilages and adjacent parts of *Heloderma suspectum* Cope,§ twice natural size. *Bo.*, basioccipital bone; *Exo.*, exoccipital; *Q.*, quadrate; *Mn.*, mandible; *Pt.*, pterygoid; *M. Pt.*, internal pterygoid muscle; VII, seventh nerve; *Col.*, columella auris; *Hst.*, hypostapedial process of auricular cartilage; *Sst.*, suprastapedial process; *Est.*, epistapedial process; *Hl.*, hypostapedial ligament; *El.*, epistapedial ligament.

* Zoötomie der Fische, p. 154.

† Such a hypothesis is suggested after inspection of Huxley's figure of these parts in Hatteria, in Anatomy of Vertebrated Animals, p. 77, Fig. A. See also American Naturalist, 1884, p. 1253; Proceeds. Amer. Philosoph. Soc., 1884, p. 41.

‡ See Proceedings Amer. Philosoph. Society, 1884, p. 41, where Peter's view is maintained.

§ I owe the specimen dissected to my friend Horatio N. Rust, who obtained it on the Gila River, Arizona.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

EIGHTH MEMOIR.

ON THE STRUCTURE OF THE BRAIN OF THE SESSILE-EYED CRUSTACEA.

ON THE STRUCTURE OF THE BRAIN OF THE SESSILE-EYED CRUSTACEA.

READ AT WASHINGTON, APRIL 14, 1884.

By A. S. PACKARD.

The following descriptions and notes have grown out of an attempt to compare the nervous system, particularly the brain and other ganglia of the head, of the eyeless species of cave-inhabiting Arthropods with their out-of-door allies. We have begun with the structure and morphology of the brain of *Asellus communis* Say as a standard of comparison with that of the blind Asellid, *Cecidotæa stygia* Pack., which is so common in the brooks of Mammoth and other caves and in the wells of Southern Indiana and Illinois. Studies of this nature are, it seems to us, well calculated to throw light on the origin of the cave forms, and to show what great modifications have been produced in these organisms by a radical change in their surroundings; consisting, as it does, mainly in the absence of light, and perhaps of the usual food, or at least the usual amount of food.

It is plain enough that the species of Cecidotæa are simply eyeless, slender, depauperated Aselli, which have originated from some one of our out-of-door species within a comparatively recent time, at least since the river-terrace epoch of the Quaternary Period. The facts bearing upon the general relations of the blind to the eyed Asellidæ, and a discussion of the change in form of the body and its appendages, and of the causes of the transformation of the species and genus, are reserved for another occasion.

My present purpose is simply to describe and depict the brain and other nerve-centers of the head of *Asellus communis* Say and *Cecidotæa stygia* Pack.

1. THE BRAIN OF ASELLUS COMMUNIS.

The nervous system of the European *Asellus aquaticus* Linn. has been referred to by Leydig and also by Sars, who published a figure of the nervous system as a whole. Leydig's "*Vom Bau des thierischen Korpers*" gives a careful and comprehensive general account of the nervous system of Arthropods, the most complete and authoritative, up to 1864, we possess, supplemented as it is by his excellent *Tafeln von vergleichenden Anatomie*, published in the same year (1864). According to Leydig, in the Isopoda (Oniscus, Porcellio) the optic lobes are very large and overlie the cerebral lobes.

In *Asellus aquaticus* the abundant fat body around the ventral cord belongs to the blood sinus which envelops the nervous cord. Of this form Leydig has little to say, remarking that he did not examine the entire ventral cord, but only sections, which agree in appearance with those of the land wood-lice.

Sar's figure of the brain of *Asellus aquaticus* is drawn on a small scale, is rather indifferent, and does not show more than the cerebral lobes and optic nerves. He evidently did not perceive the other ganglia.

Leydig's valuable figures of the brain of *Oniscus murarius* show that he did not study the nervous centers of the head by means of longitudinal sections, and that he simply dissected the brain from above, a dorsal view showing the large optic lobes to be mostly above and in front of the smaller cerebral lobes, while the ganglion, *c*, in his figure 8 (Taf. VI), which he denominates *nebenlappen*, is probably one of the antennal ganglia. The other ganglia of the head he does not represent, nor speak of in his Vergleichende Anatomie.

The other sketches of Isopod brains by Brandt and Ratzeburg, Rathke, Lereboullet, and Milne-Edwards, as well as those in our "Zoology,"* are drawn on a small scale, are in some cases rather indifferently drawn, and only represent a dorsal view, the antennal and those ganglia posterior to it being concealed from view in dissecting from above downward.†

The observations I have made are based on vertical, longitudinal sections kindly made for me by Mrs. C. O. Whitman, under the direction of Dr. C. O. Whitman. The sections were thin, clear, well-mounted in Canada balsam, in consecutive order, and made from alcoholic specimens, which had, however, been kept for several years, though the nervous system had been well preserved.

THE HISTOLOGICAL ELEMENTS OF THE GANGLIA.

Unlike the central nervous system of Vertebrates, in which there are but two kinds of nerve tissue, viz, ganglion cells and fibers, there are in the Asellidæ, as in insects and Decapods, three kinds of elements in the brain and other ganglia, viz: (1) ganglion cells; (2) nerve fibers; and (3) Leydig's *puntzsubstanz* (*marksubstanz* of Leydig and Rabl-Rückhard, and especially Dietl), which might be called the *myeloid* tissue or substance.

(1) *Ganglion cells.*—These have not, as in the brain of the lobster, a simple nucleus and nucleolus, but they usually have numerous, from 10 to 20, nuclei, the nucleolus of each nucleus readily receiving a stain and forming a distinct dark mass. They resemble those of the locust.‡ They are, as a rule, much smaller, however, than in the locust. As seen in most of the sections they appear to be spherical, being cut through transversely by the microtome, but as shown by Fig. 3*a* they are of the usual pyriform shape. In size they are very much smaller than those of the lobster and much more uniform in size, very few of the cells being twice as large as those of the average size: as already remarked, the nucleus in the ganglion cells of the American lobster are almost uniformly simple and homogeneous, with a single nucleolus. The largest ganglion cell of the lobster's brain which we have found is six times as large as the largest ganglion cell of Asellus.

The ganglion cells appear to be entirely unipolar; no bipolar or multipolar cells were observed, though special search was made for them. Nothing noticeable was observed in respect to the nerve-fibers. The *punktsubstanz*, *marksubstanz* or myeloid substance, as we may designate it, differs in its topographical relations from that of the brain of Decapoda. This myeloid substance, which seems to be peculiar to the worms, mollusks, and especially the crustacea and insects, has been most thoroughly studied by Leydig. This is the central finely-granular part of the brain, in which granules have short irregular fibers passing through them. In his *Vom Bau des thierischen Körpers*, p. 89, Leydig thus refers to it:

> In the brain and ventral ganglia of the leech, of insects, and in the brain of the Gastropods (Schnecken) I observe that the stalks (stiele) of the ganglion-cells in nowise immediately arise as nerve-fibers, but are planted in a molecular mass or *punktsubstanz* situated in the center of the ganglion, and merged with this substance. It follows, from what I have seen, that there is no doubt *that the origin of the nerve-fibers first takes place from this central punktsubstanz.*
>
> This relation is the rule. But there also occur in the nerve-centers of the invertebrates single definitely situated ganglion cells, whose continuations become nerve-fibers without the intervention of a superadded punktsubstanz.

Leydig subsequently (p. 91) further describes this myeloid substance, stating that the granules composing it form a reticulated mass of fibrillæ, or, in other words, a tangled web of very fine fibers.

> We at present consider that by the passage of the continuation of the ganglion cells into the *punktsubstanz* this continuation becomes lost in the fine threads, and on the other side of the *punktsubstanz* the similar fibrillar substance forms the origin of the axis-cylinders arranged parallel to one another; so it is as good as certain *that the single axis-cylinder derives its fibrillar substance as a mixture from the most diverse ganglion cells.*

The myeloid substance in the brain of Asellus is not however differentiated into distinct spherical masses, the *punktsubstanzballen* of Krieger (*Balken* of Dietl) or whitish ball-like masses

* Fig. 255, *Idotæa irrorata*, and Fig. 256, Serolis, drawn by J. S. Kingsley.

† Since this essay has been prepared I have obtained Dr. Bellonci's excellent memoir on the nervous system of Sphæroma, in which he figures and describes the brain and nervous system in general of that Isopod.

‡ Second Report United States Entomological Commission, ch. xi. The Brain of the Locust, 1880 (Pl. xi, Fig. 3b–3e).

which are so characteristic of the brain of the Decapod Crustacea and the insects; and in this respect there is probably a wide difference between the brain of Decapoda and Edriophthalmata.

HISTOLOGICAL TOPOGRAPHY OF THE NERVE-TISSUES.

(1) *The ganglion cells.*—These cells form a cortical layer enveloping on all or nearly all sides the central myeloid mass. The cells being distinct and more or less loosely arranged readily take a deep carmine stain, while the much more dense myeloid mass remains white and unstained.

The ganglion cells are collected into more or less definite masses, enveloped by connective tissue, the latter as it were forming a mesh, inclosing spherical masses of ganglion cells. In a vertical section, such as that represented by Figs. 2 and 3, passing through the anterior and middle part of the brain and in the horizontal section (Fig. —), while the ganglion cells are seen to be packed more or less solidly around the central myeloid portion, they are also seen to be disposed in more or less distinct lobular masses, which are inclosed by connective tissue. Seven or more distinct lobes or subspherical masses of these ganglion cells may be distinguished on each side of the brain.

As seen in Figs. 2 and 3, the uppermost or dorso-frontal lobes are the double sets filling the upper or dorsal fissure between the right and left lobes of the brain and marked *a* and *b*; *b* is divided into two sublobes, the upper (b^1) being small, flattened, and lying on the dorsal and inner edge of the central lobe. The third set is a double lobe, *c* c^1; these may be called the dorso-lateral set; they are more or less connected with the lateral lobes *d* d^1, and the latter with the externo-commissural set of lobes (*e* e^1). On the dorsal side of the brain near the base of the optic ganglia are two sets, one above and one below (*g*) the base of the optic ganglion; the exact relation of these to the others is not very plain from our sections, but they are in front of and external to the outer edge of the lobes of the brain.

The optic ganglion is enveloped by a lobulated mass of ganglion cells exactly like those of the brain proper, and these lobes (*h i k*, Fig. 27) which envelop the myeloid mass can be distinguished from the outer one at the beginning of the outer division of the nerve fibers sent to the eye from the ganglion cells.

(2) *The nerve fibers.*—The fibers arising from the ganglion cells form the commissures which unite the brain with the subœsophageal and succeeding ganglia; and also the commissures between the two cerebral lobes.

One set of fibres arise in the dorso-frontal group of ganglion cells (Fig. 3, *f b*), to become lost in the myeloid substance. The fibers are seen to pass down, and to form a part of the subœsophageal commissure, although we did not trace them to the last abdominal ganglion. Judging from Michel's observations on the commissural fibers of *Oryctes nasicornis*,* there is little doubt but that in all Arthropoda certain nerve-fibers arising in the pro cerebral lobes pass uninterruptedly to the last ventral ganglion.

It will be further seen by reference to Figs. 2, 3 (Asellus), and especially Fig. 27 (Cecidotæa), that the fibers arising from certain of the ganglion cells in lobes *c* and c^1 pass into the cerebral lobe in two directions, some connecting the two lobes, forming the transverse commissure, while others pass down and run parallel with the fibers from the dorso-frontal lobes and aid in building up the subœsophageal commissures. The latter commissure is also re-enforced by fibers from the lateral lobes *d* d^1, *e* e^1.

From what we have seen in the sections represented by the camera sketches referred to (Figs. 2, 3, and 27), and from what is known of the cells and fibers of other Arthropods, there is no doubt but that all the ganglion cells give rise to fibers, some of which at least pass directly through or above or around the myeloid substance of the cerebral lobes and form the commissures. This independence of the myeloid substance appears to be more general in the Asellidæ, at least this we would infer from Leydig's statements previously quoted. When we look at Fig. 1, which is a composition (drawn, however, with the camera) from the sections represented by Figs. 5 and 8 we see that the two main longitudinal commissures pass above the seven post-cephalic ganglia represented in the figure. Those ganglia are masses of myeloid substance, with a cortical layer of gan-

* Michels. Beschreibung des Nervensystems von *Oryctes nasicornis* in Larven, Puppens und Käferzustand. Zeits. f. wissens. Zoologie., xxxiv, 641–702. 1880.

glion cells, from which fibers arise after passing through the myeloid substance; there becoming broken up into a tangled mass of fibrillæ, which unite finally to form the fibers constituting the nerves of the appendages. Without doubt also a few commissural fibers from the procerebral lobes pass into each post-cerebral ganglion so as to afford the means to the cerebral lobes (*primi inter pares*, as happily styled by Leydig) of coördinating the nervous power of the other ganglia, their histological and morphological equivalents. It should be said that although Leydig's view as to the relations of the nerve-fibers to the myeloid substance may be the correct one, yet though it may apply to the Annelids, it may not be so general an occurrence in the Arthropods. It seems to us, though we are still open to conviction, that the transverse and longitudinal commissural fibers, which undoubtedly arise from the cortical ganglion cells, have little or nothing to do with the myeloid substance. This latter substance does not exist in the nervous system of the vertebrates, and just what its nature and function clearly are in the invertebrates has yet to be worked out. In the hands of a skillful and expert histologist, much light will yet be thrown upon this difficult subject; certainly the present writer has not the qualifications for the task. His own opinion from what little he has seen is, that the myeloid substance is the result of the splitting up into a tangled mass of very fine fibrillæ of certain of the fibers thrown off from the mono-polar ganglion cells, *i.e.*, such fibers as do not go to form the main longitudinal commissures. It should also be borne in mind that in the embryo the ganglia are composed of ganglion cells alone, with few if any primitive fibers.

MORPHOLOGY OF THE BRAIN.

The brain of the Isopods and Amphipods is a *syncerebrum*, though far less complicated than in the Decapoda. It will be remembered that Professor Lankester in his memoir on Apus designates the simple brain of that crustacean as an *archicerebrum*, while the composite brain of "all crustacea, excepting Apus, and possibly some other Phyllopods," he denominates a *syncerebrum*. In our Monograph of N. A. Phyllopoda, p. 403, we adopted the view that the brains of all Crustacea except the Phyllopoda and Merostomata were syncerebra, and we divided the syncerebrum into three types; adding that the syncerebrum of sessile eyed crustacea (Edriophthalma) was built on a different plan from that of the Decapoda.

Fig. 1 has been drawn to give a general view of the nervous centers of the head, including the first thoracic segment and its ganglion. It has been drawn with the camera from a number of sections, especially those represented by Figs. 5–8, so that it is believed to be approximately correct and not merely a schematic plan. The section passes through the head on one side of the œsophagus, which of course is not represented in the sketch; being so near the median line it does not involve the optic lobes and eyes, which, especially the latter, are on the extreme side of the body, so that these organs could not well be shown in the drawing. The general relation of the nervous system to the body walls, to the stomach and the appendages are made obvious in the sketch, and their description need not detain us. It should be borne in mind that the mouth and œsophagus open between the mandibles They are shown in Fig. 5. The end of one of the ovarian tubes is seen to overlie the pyloric end of the stomach; it does not pass into the head. The drawing of the heart is somewhat diagrammatic, as it was not well shown in the sections, but its position is believed to be approximately correct. The sympathetic nerve was not discovered.

As seen in Fig. 1, the brain or supraœsophageal ganglion is a composite mass or group of four pairs of ganglia, *i. e.*, (1) the brain proper or procerebral lobes, (2) the optic ganglia, (3) the first antennal, and (4) the second antennal lobes. These lobes are quite separate from each other in the Isopoda and Amphipoda as compared with the Decapoda.

THE PROCEREBRUM OR PROCEREBRAL LOBES.

These constitute the brain proper, and have been usually called the "cerebrum" or "cerebral lobes." As, however, they are not the homologues of the lobes of that name in Vertebrates, either structurally or functionally, we would suggest that the ganglion be termed the *procerebrum* and the individual lobes the *procerebral lobes*, not only in allusion to its position in advance of all the

other ganglia, but since it stands as the co-ordinating, regulating ganglion, the first in importance of all the ganglia.

As regards size, the procerebral lobes are more than double that of the other ganglia; they bulge out dorsally and backward, so as to conceal from above the antennal and mandibular ganglia. Plate 1, Fig. 2, represents a section through the lobes in front of the commissure, showing at *a*, *b*, the dorso-frontal group of ganglion cells, those nearest the myeloid substance sending fibers downward (*fb*) to form a part of the œsophageal commissure. At Fig. 3, a section farther back and passing through the commissure, the fibers are seen to pass directly through the myeloid substance along the inner side of the commissure. Fig. 4 represents a still more posterior section; this shows distinctly the origin of the fibers of the transverse commissure (*tr. c*) from the ganglion cells of the upper and outermost portion of the lobes. The commissure is seen to be composed of three bundles of fibers—an upper, middle, and lower or ventral; the space between the upper and middle bundles being filled with myeloid substance.

Vertical sections of the procerebral lobes are seen in Figs. 5 to 8. Fig. 5, which passes through the median line of the head, through the mouth, œsophagus, and the median line of the stomach, shows the procerebral lobe on one side of the commissure; and, below, the second maxillary and maxillipedal ganglia. Fig. 7, passing through one side of the first antennal ganglion, shows the procerebral lobe nearly separate from the antennal lobe. Fig. 8 represents a section passing through the main commissure and a portion of the procerebral lobe.

Horizontal sections from the top of the head downwards are seen in Figs. 9 to 18. Fig. 9 represents a section through the upper part of the procerebral lobes; Fig. 10, through the lobes above the transverse commissure; Fig. 11, through the entire procerebrum, near the origin of the optic ganglia and optic nerves.

THE OPTIC GANGLIA AND OPTIC NERVES.

The eyes being smaller in Asellus than in most other genera of Isopods, particularly Oniscus and Porcellio, the forms figured and described by Leydig; the optic ganglion and nerve are also much smaller, while the eyes being set farther back on the sides of the head, the ganglion and nerve are directed obliquely backward, so that a series of vertico-frontal sections pass through the brain before reaching the optic nerve. Pl. IV, Figs. 19–21, represent these organs. Fig. 19 shows the procerebral lobes, and on the left the optic ganglion and the optic nerve leading to the eye. Fig. 20 represents a section just behind the procerebral lobes, passing through the hinder edge of the cortical layer of ganglion cells. Fig. 21 is an enlarged view of the sam e. The optic lobe is divided into two parts, the inner connected with the procerebral lobe, with an abundant supply of ganglion cells, while from the smaller, outer division arise the fibers which unite to form the optic nerve, which divides at or just beyond the middle into several branches sent to the eyes. These branches are seen to end in slightly bulbous expansions among the small retina cells, forming the deep brown pigment-mass in which the lenses are imbedded.

The first antennal ganglia (Figs. 1, 7, and 12).—The relations from a side view to the other parts of the brain are seen in Figs. 1, 7, and 7*a*. It will be seen that the ganglion is much freer from the procerebral lobes than in the Decapoda. It may be seen from above, when looking down upon the brain, projecting somewhat in advance of the procerebral lobes, the first antennal nerve arising from the upper and anterior side, ascending a little at its origin, and passing horizontally into the base of the antenna. Fig. 12 represents a horizontal section through the lobes, showing the ganglion cells, the myeloid substance, and the origin of the antennal nerves.

The second antennal lobes (Figs. 1, 7, 7*a*, 14 to 16).—The second antennal ganglion lies directly beneath the upper or first antennal lobes, and appears to be slightly larger than the latter, the nerves being larger, corresponding to the much larger size of the second antenna. It will be seen by reference to Figs. 14 to 16 that the œsophagus passes between the lower part of the lobes, which are almost wholly separate. (Figs. 17 and 18, which represent sections just below that represented by Fig. 16, are introduced to show the œsophageal commissures and their ganglion cells on each side of the œsophagus.)

The first subœsophageal or mandibular ganglion (Figs. 1, 6, 7, 22, 23, *md. g.*).—This is rather larger than either of the antennal ganglia, as its relations to the brain are well seen in the sections represented by Fig. 6. By reference to the sections represented by Figs. 5 and 6, it is clearly seen to lie directly under the antennal ganglia, and to be separated from the brain proper by the short œsophageal commissures. It is therefore the first subœsophageal ganglion, givin g off but a single pair of nerves, those supplying the large tripartite mandibles.

The ganglion lies in front of the main longitudinal commissure, and in position in front of the lower side of the stomach, being situated in an inclined plane, nearer vertical than horizontal The sections represented by Figs. 22 and 23 pass through a portion of it, and in them is well seen. the mode of origin of the large mandibular nerves.

The first and second maxillary ganglia.—These are situated widely apart, neither coalescing with the other ganglia in front or behind. The first maxillary ganglion (Figs. 1, 8, 22, 23, *mx. g.*) is situated nearer the mandibular than the second maxillary ganglion, as seen in Figs. 1, 22, and 23. It lies in an inclined plane, and is much smaller than any of the other postœsophageal ganglia, as it innervates smaller appendages.

The second maxillary ganglion (Figs. 1, 8, 22, 23, *mx*^{2} *g.*) is situated next to the maxillipedal ganglion, and like that lies in a horizontal position. It is of nearly the same size but a little smaller than the ganglion next behind it, and the commissures connecting it with the maxillipedal ganglion are very short.

The maxillipedal ganglion (Figs. 1, 8, 22, 23 *mx p. g.*) is a little larger than its near neighbor, the second maxillary ganglion, inasmuch as it innervates the large maxillipedes.

At some distance behind this ganglion and situated in the first thoracic segment is the first thoracic ganglion supplying the nerves to the first pair of feet. It is a little larger than the maxillipedal ganglion.

The main longitudinal commissures (Figs. 1, 22, 23) pass over the ganglia, and are united in the head, except at two points indicated by the clear spaces in the figure, behind which point we have not traced it. Sars, however, represents the main longitudinal commissure behind the head as double.

In the section represented by Figs. 22 and 23 the limits of the mandibular and first maxillary ganglia are not definite, and they are seen to be connected by a bridge or tract of myeloid substance. Towards the second maxillary ganglion the fibers in the section are fewer and lower together, and are seen in some cases to enter the myeloid substance, but in others to pass over it. The ganglion cells of the maxillipedal ganglion are more numerous than those about the myeloid mass of the second maxillary ganglion

From the foregoing facts it will be seen that the brain of the *Asellidæ* is composed of four preœsophageal p irs of ganglia, situated at greater or less distance apart from each other, being a very loosely constructed syncerebrum compared with that of such Decapods as have been thus far examined. The mouth-parts in the *Asellidæ*, if not all Isopoda, are not innervated from a single subœsophageal ganglion, but each appendage, beginning with the mandibles, is supplied by a nerve arising from a separate ganglion. Thus there are eight ganglia of the first order in the head of these Isopods, our observations not referring to any secondary ganglia, which may or may not exist in connection with the brain or sympathetic nervous system. It will be remembered that in the Decapods, the lobster for example, the brain innervates the eyes and antennæ, while the only other ganglion in the head is the subœsophageal, from which the mouth appendages are all innervated; thus there are but two nerve-centers in the head of adult Decapods; the subœsophageal ganglia being concentrated probably during embryonic or larval life.

II. THE BRAIN OF THE EYELESS FORM CÆCIDOTÆA.

It is a matter of great interest to know just what, if any, changes take place in the brain or nerve-centers of the head of the eyeless forms related to Asellus; whether the modification is confined to the external parts of the eye, or to the optic lobes and nerves alone.

It is well known that a blind Asellus-like form is abundant in the brooks and pools of Mammoth and other caves in Kentucky and Indiana, as well as in the wells of the cavernous and adjacent

regions. The foregoing observations on the brain and eyes of the common Asellus of our brooks and ponds were made to afford a basis of comparison with the similar parts in the eyeless form.

Cæcidotæa in its external shape is seen to be a depauperate Asellus, with the body, however, much longer and slenderer than in the eyed form, and with slenderer appendages. It is not usually totally eyeless. In a number of specimens from a well at Normal, Ill., kindly sent us by Mr. S. A. Forbes, a minute black speck is seen on each side of the head in the positions of the eyes of Asellus, just above the posterior end of the base of the mandibles. In some specimens these black dots are not to be seen; in others they are visible, but fainter than in others. In twelve specimens which I collected in Shaler's Brook in Mammoth Cave I could detect no traces of eyes, and infer that most, if not all, the Mammoth Cave specimens are totally eyeless. It thus appears that different individuals have eyes either quite obsolete, if living in caves in total darkness, or, if living in wells, with eyes in different degrees of development up to a certain stage—that represented by black dots—which, however, are so easily overlooked, that we confess, after handling dozens of specimens, we did not suspect that the rudimentary eyes existed, until our attention was called to them by Dr. C. O. Whitman when he sent the slides. The European *Cæcidotæa forelii* is also said to be blind. The specimens we received through the kindness of Professor Forel, which were, unfortunately, dried and spoiled, seemed to be entirely eyeless, though special search was not made for the eye specks.

It will be seen that the eyeless Cæcidotæa differs from Asellus as regards its brain and organs of sight, in the complete loss of the optic ganglion, the optic nerve, and the almost and sometimes quite total loss of the pigment-cells and lenses.

After a pretty careful study of numerous vertical sections of the brain of *Cæcidotæa stygia* as compared with that of *Asellus communis* we do not see that there are any essential differences, except in the absence of the optic ganglia and nerves. The proportions of the procerebral lobes, of the ganglion cells, their number and distribution, the size of the transverse and longitudinal commissures are the same. The head and brain as represented is smaller than in Asellus, the form itself being considerably smaller.

Fig. 25 represents a section through the middle of the procerebral lobes, which may be compared with that of Asellus, Fig. 4. Another section a little posterior is represented by Fig. 26. Fig. 27 is an enlarged view of a section still further back, which shows that there is little, if any, difference between the brain at this point and that of Asellus represented by Fig. 3. In this section it is easy to see that the ganglion cells on each side of the procerebral lobes send fibers directly through the myeloid mass to form the transverse commissures. The section at this point does not show the fibers arising from the fronto dorsal group of ganglion cells; but traces of them are seen in Fig. 28, which represents a section corresponding to that indicated by Fig. 3.

Careful examination of the sections passing behind the procerebral lobes and œsophageal commissures failed to show any traces of the optic ganglion of either division, or of the ganglion cells and myeloid substance composing it. Every part connected with the optic ganglia seems to be totally abolished. The same may be said of the optic nerve throughout its length. The amount of time spent in examining the numerous well cut, thin, and beautifully mounted sections made by Dr. Whitman, or under his direction, enables us to affirm positively that the entire nervous portion of the optical organs are wanting. And we are glad to add that Dr. Whitman also observed to us the absence of the optic nerves.

With the eye itself it is different. The modification resulting from a life in total darkness has left traces of the eye, telling the story of degeneration and loss of the organs of sight, until but the merest rudiments of the eye remain as land marks pointing to the downward path in degradation and ruin taken by the organs of vision as the result of a transfer to a life in total or partial darkness, as the case may have been, in the well-inhabiting or cave-dwelling individuals.

Fig. 29 represents a section through the head of *Cæcidotæa stygia* behind the procerebral lobes and œsophageal commissures, showing the absence of any traces of the optic ganglia or optic nerves, but indicating the rudiments of the eye, showing that the pigment mass of the retina and the lenses exist in a very rudimentary condition, while the optic nerve and ganglion are entirely aborted.

Figs. 30 and 31 represent enlarged views of the rudimentary eye of two different specimens of *C. stygia* from Mammoth Cave. In the sections represented by Fig 30 *a b* we see that the number of facets has been reduced apparently to two (*b*), the rudimentary lenses being enveloped by a black pigment mass. This section, examined by Tolles' $\frac{1}{5}$ A, is magnified and drawn to exactly the same scale as that of the eye of Asellus represented by Fig. 21. In that figure may be seen the normal size of the lenses and of the retina cells. It will be seen that in Cæcidotæa the retina cells are broken down and have disappeared as such, and that the rudimentary lens (or the hyaline portion we suppose to be such) which the retinal pigment incloses is many times smaller than in the normal eye of Asellus.

On comparing the eyes of the two specimens as shown in Figs. 31*a* and 32*a*, it will be seen that the eyes in one are considerably larger than in the other specimen. Fig. 32*b* shows that in the eye of this individual there were at least four lenses, if not more, not included in the section. At the point indicated by 32*d* on the edge of the eye one lens is indicated (though the divisions are wanting), not wholly concealed by the pigment of the retina; a more magnified view is seen at Fig. 32*e*. The four sections *a–d* passed through the eye, the section in front and behind not touching the eye itself.

It thus appears from the observations here presented that the syncerebrum of the blind Cæcidotæa differs from that of the normal Asellus in the absence of the optic ganglia (both divisions) and the optic nerves, while the eyes are exceedingly rudimentary, the retinal cells being wanting; the black pigment mass inclosing very rudimentary minute lens-cells, which have lost their transverse zonular constriction or division; the entire eye of Cæcidotæa finally being sometimes wanting, but usually microscopic in size, and about one-fifth as large as that of the normal Asellus.

The steps taken in the degeneration or degradation of the eye, the result of the life in darkness, seems to be these: (1) the total and nearly or quite simultaneous loss by disuse of the optic ganglia and nerves; (2) the breaking down of the retinal cells; (3) the last step being, as seen in the totally eyeless form, the loss of the lens and pigment.

That these modifications in the eye of the Cæcidotæa are the result of disuse from the absence of light seems well proved; and this, with many parallel facts in the structure of other cave Crustacea, as well as insects, arachnids, and worms, seems to us to be due to the action of two factors: (a) change in the environment; (b) heredity. Thus we are led by a study of these instances, in a sphere where there is little, if any, occasion for struggling for existence between these organisms, to a modified modern form of Lamarckianism to account for the origination of these forms, rather than to the theory of natural selection, or pure Darwinism as such.

BIBLIOGRAPHY OF WORKS ON THE NERVOUS SYSTEM OF CRUSTACEA.

GENERAL.

BELLONCI, G. Intorno alla struttura e alle connessioni dei lobi olfattorii negli Artropodi superiori e nei vertebrati. 2 tav. 4. Ivi, 1882.

BERGER. Untersuchungen über den Bau des Gehirns und der Retina der Arthropoden. Arbeiten des zool. Instituts zu Wien, Heft 2.

——— Nachtrag zu den Untersuchungen über den Bau des Gehirns und der Retina der Arthropoden. Ibid., Heft 3.

EHRENBERG. Beobachtung einer bisher unerkannten Structur des Seelenorgans. 1836.

GRENACHER, H. Untersuchungen über das Seeorgan der Arthropoden, insbesondere der Spinnen, Insecten, und Crustaceen. Göttingen, 1879.

HANNOVER. Recherches microscopiques sur le système nerveux. 1857.

HELMHOLTZ. De fabrica systematis nervosi evertebratorum. Diss. Berolini, 1842.

LEYDIG. Lehrbuch der Histologie des Menschen und der Thiere. 1857.

——— Vom Bau des thierischen Körpers. Erster Band. 1864.

——— Tafeln zur vergleichenden Anatomie. Erstes Heft. 1864.

MILNE-EDWARDS, H. Histoire naturelle des Crustacés. Tom. i–iii. Paris, 1834–1840.

REMAK. Ueber d. Inhalt der Nervenprimitivröhren. Archiv f. Anat. u. Phys., 1843.

WALTER. Mikroscopische Studien über das Centralnervensystem wirbelloser Thiere. 1863.

CIRRIPEDIA.

BRANDT, E. Ueber das Nervensystem der Lepas anatifera (anatomisch-histologische Untersuchung). Mélanges biologiques, vii. Bull. de l'Acad. imp., xv. St. Petersburg. June, 1870.

CUVIER, G. Memoire sur les Mollusques. Paris, 1817.

CLAUS, C. Die Cypris-ähnliche Larve (Puppe) der Cirripedien und ihre Verwandlung in das festitzende Thier. Schriften der Gesellschaft z. ges. Naturw. Marburg. Supp.-Heft v. Marburg u. Leipzig, 1869. (Brain of larva of Conchoderma.)

DARWIN, C. A monograph of the sub-class Cirripedia. 1851, 1853.

KINGSLEY, J. S. Barnacles. American Naturalist, xi, 102. 1877, and Packard's Zoology, Fig. 228. (Nervous system of Lepas fascicularis.)

MARTIN ST.-ANGE. Mémoire sur l'organisation des Cirripedes. Paris, 1835.

WYMAN, J. Proceedings Boston Soc. Nat. Hist. Amer. Jour. Sci. Arts, xxxix, 182. 1840. (Nervous system of *Otion cuvieri*.)

ENTOMOSTRACA.

CLAUS, C. Beiträge zur Kenntniss der Entomostraken. Marburg, 1860. (Nervous system of *Saphirina fulgens*.)

——— Die freilebenden Copepoden. Leipzig, 1863. (Nervous system of Copilia, Corycaeidae, Saphirella, Calanella, Cetochilus, Saphirina, Dias longiremis.)

——— Ueber die Entwickelung, Organization und systematische Stellung der Arguliden. Zeits. wissens. Zool., xxv, 1875. (Nervous system of Argulus foliaceus and coregoni.)

GEGENBAUR. Mittheil. üb. d. Organization von Phyllosoma und Sapphirina. Archiv f. Naturgeschichte, Taf. v, fig. 1, 1858.

NORDMANN. Mikrographische Beiträge. Heft 2, 1832.

ZENKER. Anat. syst. Studien über die Krebsthiere. Archiv f. Naturg., Taf. vi, fig. 13, 1854.

BRANCHIOPODA.

BRONGNIART, A. Mémoire sur la Limnadia. Mém. du Muséum, vi, 1820.

CLAUS, C. Zur Kenntniss des Baues und der Entwicklung von Branchipus stagnalis und Apus cancriformis. Abh. k. Gesell. Wissens., Göttingen, Bd. viii, 1873. (Brain of young B. stagnalis and A. cancriformis.)

——— Zur Kenntniss der Organization und des feineren Baues der Daphniden und verwandter Cladoceren. Zeits. wissens. Zool., xxvii, 1876. (Brain of Daphnia.)

——— Untersuchungen zur Erforschung der genealogischen Grundlage des Crustaceen-Systems. Wien, 1876. (Nervous system of young Estheria ticinensis.)

GRUBE, A. E. Bemerkungen über die Phyllopoden. Archiv f. Naturg., xix. Berlin. 1853. Nervous system of Limnetis.

JOLY. Recherches zool., anat. et phys. sur l' Isaura cycladoides. Annales d. Sc. Nat., 1842.

LANKESTER, E. R. Observations and reflections on the appendages and on the nervous system of Apus cancriformis. Quart. Jour. Micr. Sc., April, 1881.

LEYDIG, FR. Naturgeschichte der Daphniden. Tübingen 1860. (Brain of several species of Daphnia, Sida, Lynceus, Pasithea, Bosmina, Polyphemus, and Bythotrephes.)

——— Verg. Anatomie. 218, 1864.

——— Ueber Artemia salina u. Branchipus stagnalis. Beitr. z. anat. Kenntniss dieser Thiere. Zeits. f. wissen. Zoologie, 1851.

MÜLLER, P. E. Danmarks Cladocera. Kroyer's Tidsskrift, Ser. iii, Bind v, 1868–9. (Brain of Bythotrephes, Podon, Evadne, Leptodera.)

PACKARD, A. S., jr. Monograph of the Phyllopod Crustacea of North America, with remarks on the order Phyllocarida. Twelfth Annual Rep. U. S. Geol. Surv. Terr., 1883. (Nerv. system of Apus lucasanus. Pl. xxxii, fig. 1. In this figure the lower loop between the liver and œsophagus should be removed and the commissure extended from above to take its place. Nervous system of Estheria mexicana and Branchipus vernalis. Plate xxxiii.)

SPANGENBERG, FR. Bemerkungen zur Anatomie der Limnadia Hermanni Brongn. Zeits. wissens. Zool., xxx, Supp., 171, 1878. (Nervous system described at length.)

WEISMANN, A. Ueber Bau und Lebenserscheinungen von Leptodora hyalina Lilljeborg. Zeits. wissens. Zool., xxiv, 1874.

ZADDACH, E. G. De Apodis cancriformis. Anatome et Historia evolutionis. Bonnae, 1841. (Nervous system of Apus cancriformis.)

PHYLLOCARIDA.

CLAUS, C. Untersuchungen zur Erforschung der genealogischen Grundlage des Crustaceen-Systems. Wien, 1876. (Nervous system of Nebalia geoffroyi.)

PACKARD, A. S., jr. Monograph of the Phyllopod Crustacea of North America, with remarks on the order Phyllocarida. Twelfth Ann. Rep. U. S. Geol. Surv. Terr., 1883. (Brain and nervous system of Nebalia bipes.)

EDRIOPHTHALMA.

AUDOUIN ET MILNE-EDWARDS. Annales des Sc. Nat., xiv, 1828. (Nervous system of Talitrus and Cymothoa.)
BELLONCI, G. Sistemo nervoso e organi dei sensi della. Sphæroma serratum. Constar, Roma, 1881.
BRANDT UND RATZBURG. Medicinische Zoologie, 1829. (Nervous system of Oniscus.)
BRUSELIUS. Beitrag zur Kenntniss vom inneren Bau der Amphipoden. Archiv f. Naturgesch., 1859.
CLAUS, C. Untersuchungen zur Erforsch. d. Genealog. Grundlage des Crust. Syst. Wien, 1876. (Brain and organ of hearing of Oxycephalus figured.)
DE LA VALLETTE, G. De Gammaro puteano. 1857.
DOHRN, A. Naturgeschichte der Caprella. Zeits. wissens. Zool., xvi, 245, 1863. (Includes that of Cyamus ceti.)
——— Untersuchungen über Bau und Entwicklung der Arthropoden, 4. Entwicklung und Organization un Praniza (Anceus) maxillaris. 5. Zur Kenntniss des Baues von Paranthura costana. Zeits. wissens. Zoologie, xx, 1870.
FREY UND LEUCKART. Lehrbuch der Zootomie. 195, 1847. (Nervous system of Idothea and Caprella.)
GAMROTH, A. Beiträge zur Kenntniss der Naturgeschichte der Caprellen. Zeits. wissens. Zoologie, xxxi. (Brain of Caprella æquilibra.)
GERSTAECKER, A. Bronn's Classen und Ordnungen der Thierreichs. Arthropoda. Abth., ii, 1880.
KINGSLEY, J. S. Figures of the nervous system of Idotæa irrorata and Serolis guadichaudi. In Zoology for high schools and colleges, by A. S. Packard, Jr. New York, 1878.
LEREBOULLET, A. Mém. sur la Ligidia Persoonii Brdt. Annales des Sc. Nat., xx, 1843.
——— Mémoire sur les Crustacés de la Famille des Cloportides qui habitent les environs de Strasbourg. Mém. Soc. d'Hist. Nat. Strasb. Strasbourg, 1853. (Nervous system and brain of Oniscus murarius.)
LEYDIG, FR. Vom Bau des thierischen Körpers. Handbuch der vergleichenden Anatomie. Bd. 1. Tübingen, 1864. (Nervous system of Asellus aquaticus, Porcellio scaber and Oniscus murarius, Armadillo described.)
——— Taplen zur Vergleichenden Anatomie. Heft 1, Zum nervensystem und den Sinnesorganen der Würmer und Gliederfüssler. Tübingen, 1864. (Nervous system of Porcellio scaber; brain and eyes of Oniscus murarius figured.)
PAGENSTECHER. Phronima sedentaria. Ein Beitrag z. Anat. u. Phys. dieses Krebses. Archiv für Naturgeschichte, 1861.
RATHKE, H. Nueste Schrift. d. Naturg. Gesellsch, in Danzig, 1820. (Nervous system of Idothea.)
——— De Bopyro et Nereide. 1837.
RENTSCH. Homoiogenesis. Beitr. z. Natur- u. Heilkunde, 1860. (Nervous system of Gammarus ornatus.)
——— Nova Acta Nat. Cur. xx, 1843. (Aega, etc.).
ROUSSEL DE VANZÈME. Annales des Sc. Nat., i, 1834. (Nervous system of Cyamus, "beautiful figure" Leydig.)
STRAUSS-DURCKHEIM. Mém. sur les Hiella. Mém. du Muséum d'hist. Nat., xviii, 1829.
TREVIRANUS. Vermischte Schriften Anat. u. phys. Inhalts. 1816. (Nervous system of Oniscus and Cyamus.)
SARS, G. O. Histoire naturelle des Crustacés d'eau douce de Norvége. 1e Liv. Les Malacostraces. Avec 10 Pl. Christiania, 1867. 4°. (Nervous system of Asellus aquaticus.)

STOMAPODA.

BELLONCI. Morfologia del sistema nervoso centrale della squilla mantis. Annali del Museo civico di storia naturale di Genova, XII, 1878. (See also under GENERAL, Bellonci's Memoir on Squilla in Intorno alla struttura artropodi, &c.)
——— Nuove Ricerche sulla Struttura del Ganglio ottico della *Squilla mantis*. Mem. Acad. Sc. Inst. Bologna, iii, 419–426, Tav. 1–3, 1882.
CUVIER, G. Leçons sur l'anatomie comparée, 1809.
DELLE CHIAJE. Descrizione e notomia degli animali invertebrati della Sicilia citeriore. Napoli, 1841–'44.

SCHIZOPODA.

FREY. De Mysidis flexuosi anatomi, 1846.
——— and LEUCKART. Beiträge zur Kenntniss wirbellos. Thiere, 1847.

DECAPODA.

BELLONCI, G. Sui lobi olfattorii del Nephrops norvegiens. Memorie dell'Acc. d. Scienze di Bologne, 1880.
BRANDT UND RATZEBURG. Medizinische Zoologie, 1829.
CARUS, G. Lehrbuch der vergleichende Zootomie, 1818. Zweite Aufl., 1834.
CUVIER, G. Leçons d'anatomie comparée. Paris, 1809.
DIETL, M. J. Die Organization des Arthropodengehirns. Zeits. f. wissen. Zoologie, xxvii, p. 488, 1876.
HAECKEL, E. Ueber d. Gewebe des Flusskrebses. Archiv f. Anat. u. Phys., 1857.
HANNOVER. Recherches microscopiques sur le systeme nerveux, 1844.
HOME, E. On the internal structure of the human brain, when examined in the microscope, as compared with that of fishes, insects, and worms. Phil. Trans., 1824, Pl. 4, Fig. 3.
KRIEGER, K. R. Ueber das centrale Nervensystem des Flusskrebses. Zool. Anzeiger, 1. Jahrg., Nr. 15, 1878.

KRIEGAR, K. R. Ueber das Centralnervensystem des Flusskrebses. Zeits. f. wissen. Zoologie, xxxiii, p. 527, Jan. 23, 1880.

KROHN. Ueber die Verdauungsnerven des Krebses. Isis, 1834.

LEMOINE. Recherches pour servir a l'histoire des systemes nerveux musculaire et glandulaire de l'écrevisse. Ann. des sciences nat. 5e sér. Zoologie, ix, 1868.

MÜLLER, JOH. Acta Acad. Cæsareo-Leopold., XIV.

NEWPORT, G. Phil. Trans., 1834. (Nervous system of Homarus.)

OWSJANNIKOFF, PH. Recherches sur la structure intime du système nerveux des Crustacés et principalement du homard. Ann. des sciences nat. 4e sér., Zoologie, xv, 1861.

——— Ueber die feinerer Structur des Kopfganglions bei den Krebsen, besonders beim *Palinurus locusta*. Mém. Acad. imp. sc. St.-Pétersbourg, vi, No. 10, 1863.

OWEN, R. Lectures on the comp. anatomy and physiology of the invertebrate animals, 1843.

PACKARD, A. S. Zoology for colleges, 1879. Brain of Cambarus and the blind *Orconectes pellucidus*, briefly described and figured. Fig. 269.

SCHLEMM. De hepate ac bile Crustaceorum, 1844.

SWAN. Comparative anatomy of the nervous system, 1835.

VALENTINE. Nova Acta Nat. Curios. xviii, 1836.

VIALLANES, H. Études histologiques et organologiques sur les Centres nerveux et les organes des Sens des Animaux articutes. Premier mémoire. Le Ganglion optique de la Langouste (*Palinurus vulgaris*). Annales des Sciences Nat. vie serie. Tom. xvii. July, 1884, 1–74.

WEBER, E. H. De aure et auditu hominis et animalium, 1820.

WILL. Archiv für Anat. u. Phys., 1844.

WILLIS. De anima brutorum, 1674.

YUNG, E. Recherches sur la structure intime et les fonctions du systeme nerveux central chez les Crustacés décapodes. Archives de Zoologie exp. et gén., vii, 1878.

——— De la structure intime du système nerveux central des Crustacés décapodes. Comptes rendus, lxxxviii, 1879.

MEROSTOMATA.

MILNE-EDWARDS, A. Recherches sur l'anatomie des Limules. Annales des sciences nat. Zoologie, xvii, Nov., 1872.

PACKARD, A. S., jr. On the development of the nervous system in Limulus. American Naturalist, ix, 422. July, 1875.

——— Structure of the eye of Limulus. Amer. Nat., xiv, 212. March, 1880.

——— Internal structure of the brain of Limulus. Amer. Nat., xiv, 445. June, 1880.

——— The anatomy, histology, and embryology of Limulus polyphemus. Anniversary memoirs of the Boston Soc. Nat. Hist. 1880.

VANDER HOEVEN, J. Recherches sur l'histoire naturelle et l'anatomie des Limules. Leyde, 1838.

EXPLANATION OF PLATES.

PLATE I.—ASELLUS COMMUNIS.

Fig. 1. Longitudinal section through the head on one side of mouth and œsophagus, showing the brain or procerebrum (*pcm*), first and second antennal ganglia; mandibular, first and second maxillary, the maxillipedal ganglia and nerves passing to the antennæ and mouth-parts × 1½ inch A.

Fig. 2. Section through the procerebral lobes in front of the optic nerves × ½ A.

PLATE II.—ASELLUS COMMUNIS.

Fig. 4. Section of the procerebrum posterior to Fig. 3, × ½ A.

Fig. 3. Section through procerebrum and main commissure × ½ A, 3*a*, ganglia cells from lobe *b*. × ⅛ C.

Fig. 5. Section through the median line of the head, involving the œsophagus and one of the procerebral lobes.

Fig. 6. Section through the head. × ½ A.

Fig. 7. Section of the head passing through one side of the first antennal ganglion and showing the origin of the first antennal nerve; also the second antennal ganglion, and mandibular ganglion (*md.g*) × ½ A.

Fig. 7*a*. Section passing near 7 and through the main commissure.

PLATE III.—ASELLUS COMMUNIS.

Fig. 8. Section passing through the main commissure from the procerebral to the 1st pedal ganglion.

Fig. 9–18. Horizontal sections from the top of the head downwards × ½ A.

Plate IV.—Asellus communis.

Fig. 19. Transverse section of the head through the procerebral lobes and through the eyes and optic nerves and commissures × ½ A.

Fig. 20. A section back of the procerebrum passing through the optic ganglion, optic nerve and eye.

Fig. 21. Same section as in Fig. 20, enlarged × ½ A. *re*, retinal cells; *op, n*, optic nerve; *h, i, k*, masses of ganglion cells.

Fig. 22. Horizontal section through the main commissures and the first and second maxillary ganglia, and maxillipedal ganglia, and showing the origin of the mandibular nerves. × ½ A.

Fig. 23. The same section as in Fig. 22, enlarged. × ½ A.

Plate V.—Cæcidotæa stygius.

Fig. 25. Transverse section through the procerebrum and commissures. × ½ A.

Fig. 26. Section a little posterior to that of Fig. 25. × ½ A.

Fig. 27. Enlarged sketch of section still farther back. × ½ A.

Fig. 28. Enlarged sketch of section still farther back. × ½ A.

Fig. 29. Section behind procerebrum and showing the rudimentary eye, but entire absence of the optic ganglion and optic nerve.

Fig. 30. Section through the eye. × ½ A.

Fig. 31. Section through the eye of another individual. × ½ A. *c*, lens. × ½ *c*.

Fig. 32. Section through a ventral ganglion.

Fig. 33. Section through a ventral ganglion.

Fig. 34. Section through a ventral ganglion under the stomach.

Fig. 35. Section through a ventral ganglion under the stomach.

Note.—All the figures drawn by the author with the camera lucida.

○

Plate 1

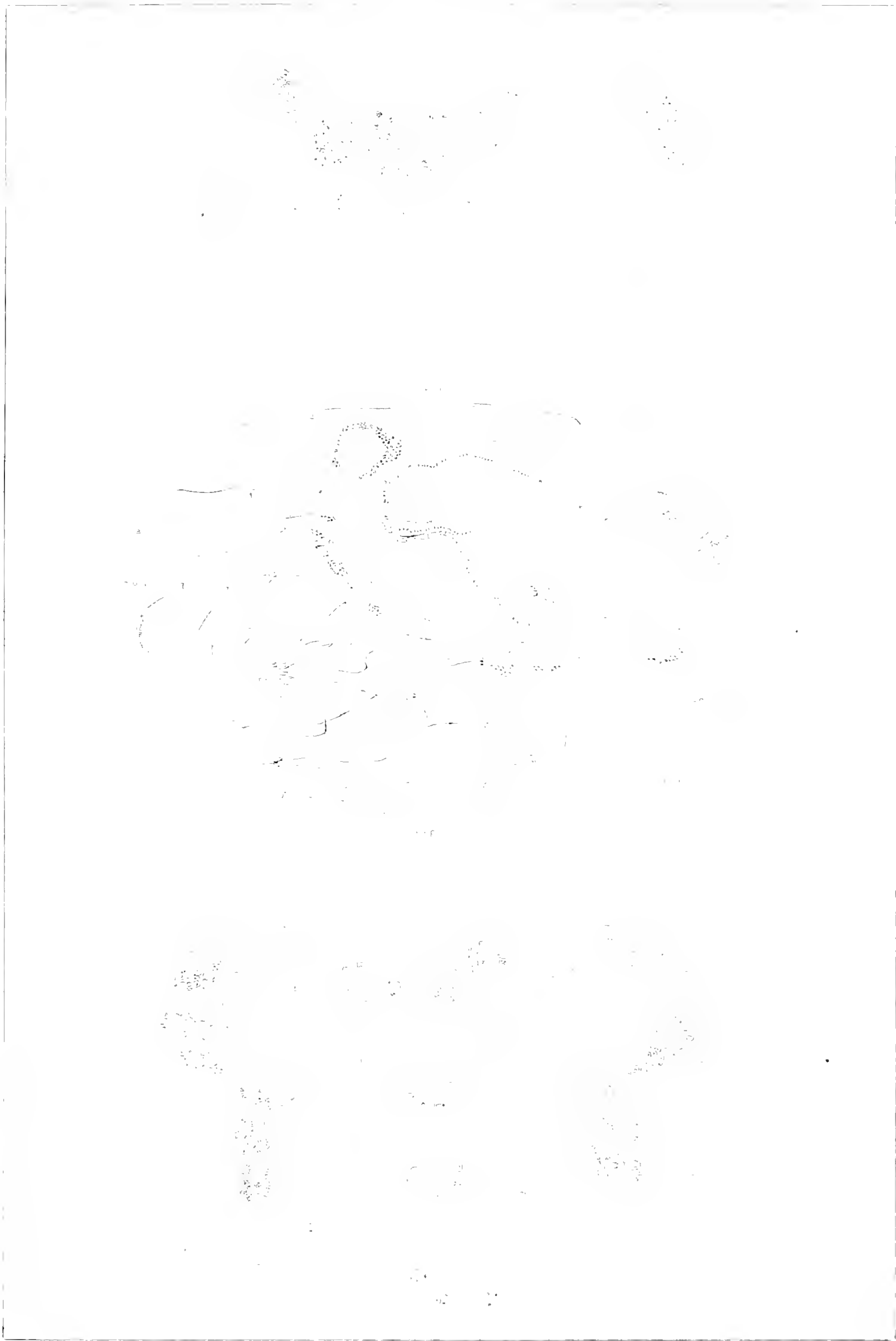


Plate III

Plate IV

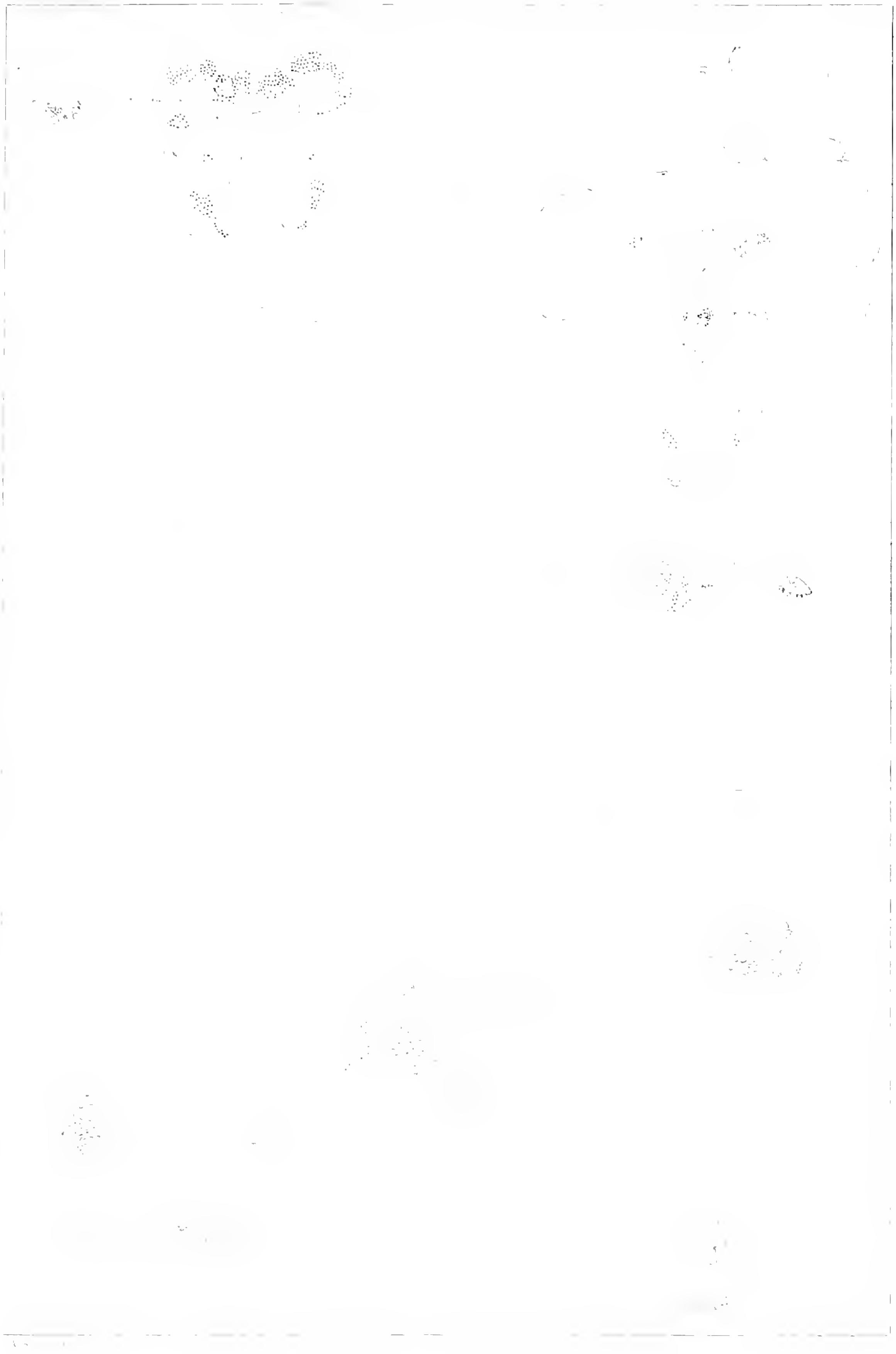
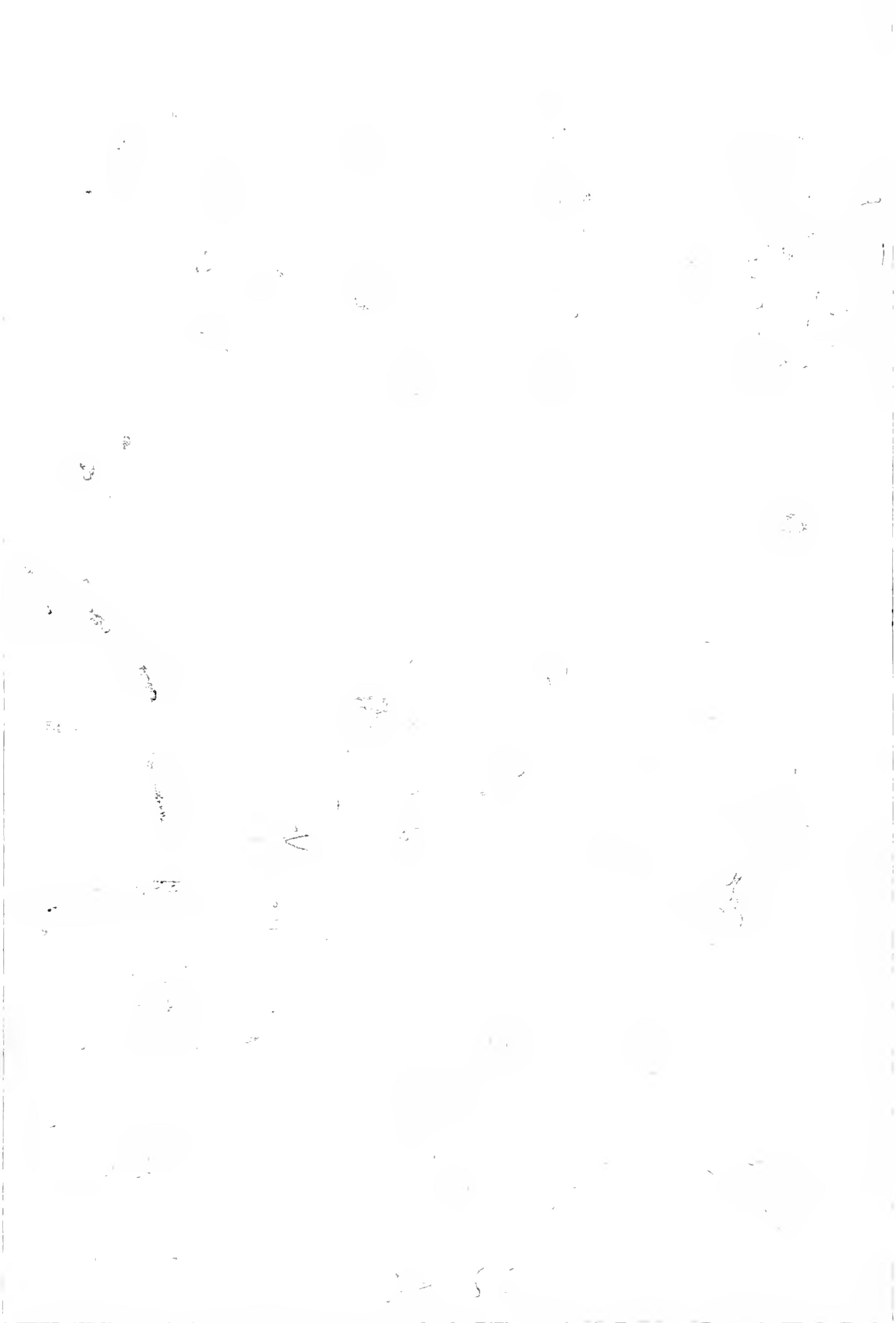


Plate X



A S Packard del.

MEMOIRS

OF THE

NATIONAL ACADEMY OF SCIENCES.

Volume III.

PART 2.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1886.

MEMOIRS.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

NINTH MEMOIR.

CONTRIBUTIONS TO METEOROLOGY.

PREFACE.

Fifty years ago, when a tutor in Yale College, I became greatly interested in Redfield's investigations respecting the laws of storms; and from that time to the present day I have never lost my interest in meteorological phenomena. In 1836 I was appointed professor of natural philosophy in Western Reserve College, and was sent to Europe to purchase instruments for my department. Among my purchases was a superior set of meteorological instruments, and on my settlement in Ohio I commenced a meteorological journal, embracing daily observations of the barometer, thermometer, &c., and I also made hourly observations for thirty-six hours at the equinoxes and solstices, according to the scheme proposed by Sir John Herschel. In October, 1837, a hurricane of considerable violence passed within 5 miles of Hudson, and I improved the opportunity to make a careful survey of its track, with special reference to deciding between the conflicting views of Redfield and Espy, but the materials for this purpose were not as complete as I had expected. In order to obtain fuller materials for this purpose I resolved to select some storm of unusual violence and collect all the information possible respecting it, and to make a thorough examination of its phenomena. I selected the storm of December 20, 1836, and succeeded in obtaining a considerable mass of observations relating to it. The results of this investigation were published in the Transactions of the American Philosophical Society, and seemed to show that neither the views of Redfield nor Espy were wholly correct, and that much remained to be learned respecting the laws of our winter storms. I found it impossible to obtain observations respecting the storm of December, 1836, which would enable me to make so complete an investigation as I desired, and I waited in the hope of being more successful with some future storm.

In February, 1842, a tornado of unusual violence passed within 20 miles of Hudson. As soon as I received the news, I started out with chain and compass to make a thorough survey of the track, and succeeded to my entire satisfaction. As the tornado passed over a forest of heavy timber, I had the best opportunity to learn the direction of the wind from the prostrate trees; and by measuring the direction of the trees as they lay piled one upon another, I determined the successive changes in the direction of the wind. The facts demonstrated incontestably that the movement of the wind was spirally inward and upward, circulating from right to left about the center of the tornado. This tornado was but an incident in a great storm which swept over the United States, and I resolved to collect all the information possible respecting the general storm. In this attempt I met with fair success, and in discussing the observations I adopted methods which are now familiar to all the world, but which were new to me, and which, so far as I know, had not at that time been employed by any other person. The results were published in the Transactions of the American Philosophical Society. This investigation showed conclusively that Redfield was mistaken in supposing that in all great storms the wind revolves in circles about the center; and also that Espy was mistaken in supposing that the air moves toward the center in the direction of radii.

After completing this investigation, I desired to apply my new methods of research to another violent storm, but the labor and expense involved in collecting my materials induced me to wait, hoping that as the number of observers increased more abundant materials might be obtained, and with a less expenditure of time and money. In 1856, during a somewhat extended tour through Europe, I improved the opportunity to collect observations respecting a storm which prevailed in Europe soon after the American storm of December, 1836, and which some persons supposed to have been connected with the American storm. On my return to the United States these observations were carefully discussed and the results were published by the Smithsonian Institution.

Years rolled on, and the favorable opportunity which I had looked for to enable me to resume my investigations of the phenomena of storms did not come. The Smithsonian Institution had indeed organized a large body of meteorological observers, but most of the observers had no barometer, and many of the barometers which were used were unreliable. At length the Signal Service was organized, and now came the opportunity for which I had been waiting thirty years, but had almost despaired of living to witness. As soon as I had obtained one daily weather map for two years I commenced a careful examination of these maps, for the purpose of deducing from them general laws. As the observations multiplied, I was enabled to undertake the investigation of new questions, and the results are contained in a series of papers published in the American Journal of Science, and entitled "Contributions to Meteorology." These papers have attracted considerable attention in Europe. The first nine papers were translated into French by M. Brocard, and were published in Paris by the late Abbé Moigno, under the title of Météorologie Dynamique A very full abstract of several of these papers has been published in Italian by Dr. Ciro Ferrari, of the meteorological office at Rome, in a pamphlet of 92 pages, with numerous plates. Notices of most of the papers have from time to time appeared in various scientific journals of Great Britain and the Continent of Europe.

The subjects investigated in these contributions were taken up without any regard to systematic order, and the later results, having been derived from a much greater mass of materials, will sometimes be found not to harmonize entirely with the results published in my earlier papers. Under these circumstances it has been thought desirable to revise the entire series of papers, and reduce them to a more systematic form, improving the opportunity to combine new researches on points heretofore neglected, and to deduce all results from the entire series of observations now available, not only from the United States but from Europe and other parts of the world. The present memoir contains the first chapter of this revision, and it is designed that other chapters shall follow as rapidly as my strength will permit.

E. L.

CONTRIBUTIONS TO METEOROLOGY.

By Elias Loomis.

AREAS OF LOW PRESSURE—THEIR FORM, MAGNITUDE, DIRECTION, AND VELOCITY OF MOVEMENT.

1. The pressure of the atmosphere is continually changing. In the middle and higher latitudes of the Northern Hemisphere these changes are very great and sometimes very sudden. In the northern part of the United States the barometer frequently rises a half inch above its mean height, and it has been known to rise a whole inch above the mean. It frequently sinks a half inch and sometimes more than an inch below the mean. These changes take place simultaneously over regions of vast extent. In order to exhibit these phenomena in the simplest manner, we draw lines connecting all those places where the pressure at a given instant is the same. Such lines are called lines of equal pressure, or isobaric lines, or simply *isobars*. Plate I shows the isobars for the United States on the 15th of January, 1877, at 4^h 35^m P. M., Washington time, the isobars being drawn at intervals of one-tenth of an inch. It must be understood that the barometric observations here represented are not the actual readings of the barometer, but a correction has been applied to all of them to reduce them to sea-level. We see that the region over which the pressure was less than 30 inches, is of an elongated form, about 1,000 miles in diameter, measured in a direction from NW. to SE., and about 1,800 miles in diameter measured in a direction from SW. to NE. This region, over which the pressure is less than the mean, is called an area of low pressure; the point where the barometer is lowest is called the center of the low area; and on the Signal Service maps this center is marked Low.

2. If the atmosphere were of uniform density from the surface of the earth to its upper limit, these differences of pressure would indicate differences in the height of the atmosphere; and if an observer could be elevated above the earth so as to see the whole area of low pressure at one view, he would notice a depression in the upper surface of the atmosphere somewhat similar to that produced when a vessel of water is rotated rapidly about a vertical axis. The upper surface of the atmosphere over the low area would appear depressed below a horizontal surface, and would appear to slope upwards from the low center. This slope is called the atmospheric gradient, or *barometric gradient*, and the steepness of the slope is indicated by the increase of height in a given distance, or the change of barometric pressure in a given distance. The unit of distance now generally adopted is a degree of the meridian, or 60 nautical miles. We notice that on Plate I the gradient is not the same in all directions from the low center, but is steepest on the northwest side. Here the isobars are crowded close together, their average distance from each other being 43 nautical miles. The change of pressure for a distance of 60 nautical miles, measured in a direction perpendicular to the isobars, is 0.14 inch, and this is the barometric gradient for that part of the low area. This is a very steep gradient, and only occurs in the case of violent storms.

3. The direction of the wind within this low area is indicated by arrows, and the velocity of the wind by the number of feathers on the tail of each arrow; one feather indicating a velocity not exceeding 5 miles per hour; two feathers indicating a velocity from 5 to 10 miles per hour; three feathers a velocity from 10 to 15 miles, and so on up to ten feathers, indicating a velocity

from 45 to 50 miles per hour. We see that (with a few exceptions, which may generally be ascribed to local causes) the winds all have a tendency inward toward the low center, and at the same time they circulate around this center in a direction contrary to the motion of the hands of a watch. We also see that the average velocity of the wind is greatest on the northwest side, where the gradient is steepest, and that the winds are generally feeblest where the gradients are least. This remark must not be construed as applicable rigorously to the observations at each locality, but rather to the average velocity of the wind over a considerable district.

4. This storm was attended by a great fall of rain and snow, the precipitation at Louisville, Ky., having been 1.56 inches during the nine hours preceding $4^h\ 35^m$ P. M., January 17, and 2.13 inches during the seventeen hours preceding $4^h\ 35^m$ P. M. The greatest rain fall was pretty near the center of low pressure, and was situated upon the southeast side of it. The area over which there was a fall of at least a quarter of an inch of water (in the form of rain or snow) was 650 miles in diameter, measured in a direction from NW. to SE.; and 1,250 miles in diameter, measured in a direction from SW. to NE.

5. The contrasts between the temperatures prevailing on the opposite sides of the low center were uncommonly great. On the northwest side the thermometer was extremely low, viz, at Pembina, $-22°$; at Breckenridge, $-21°$; at Fort Garry, $-18°$, and at Yankton, $-13°$. On the southeast side of the low center the temperature was unusually high for midwinter, viz, at Key West, 85°; at Punta Rassa, 74°; at Jacksonville and New Orleans, 73°; at Savannah and Augusta, 70°. Thus at the same instant of time, from Key West to Breckenridge (distant 1,700 miles), the difference of temperature was 106°, showing an average difference of one degree for each 16 miles of distance. In the neighborhood of the low center, the contrasts of temperature were still more remarkable. At Memphis, the thermometer stood at 61°, showing a difference of temperature from Yankton to Memphis (distance 690 miles) amounting to 74°, being an average difference of one degree for each 9 English miles of distance. It will be seen hereafter that the phenomena here noticed are in their main features characteristic of the violent storms of the United States, particularly during the colder months of the year.

6. The isobars represented on Plate 1 are not circles. Occasionally we find examples in which the isobars about a low center approach more nearly to a circular form, but the Signal Service maps do not, on an average, show more than one case in a year in which the isobars do not differ sensibly from circles.

From an actual measurement of the greatest and least diameters of the isobars represented on the Signal Service maps for $7^h\ 35^m$ A. M. during a period of three years, the following average results have been obtained:

The average ratio of the longest diameter of the isobars to the shortest was 1.94.

$$\left.\begin{array}{r}\text{In } 59\\ 33\\ 11\\ 3\end{array}\right\}\begin{array}{c}\text{per cent. of the whole number of cases,}\\ \text{the ratio of the longest}\\ \text{diameter to the shortest}\\ \text{was greater than}\end{array}\left\{\begin{array}{l}1.5\\ 2\\ 3\\ 4\end{array}\right.$$

7. The longest diameter of the isobars may be turned in any azimuth, but it is most frequently directed towards a point somewhat east of north. The following table shows the number of cases in a hundred in which the longest diameter of the isobars was directed towards each of the ten-degree intervals of azimuth, counting from the north point around by east towards the south:

Azimuth.	Cases per cent.	Azimuth.	Cases per cent.	Azimuth.	Cases per cent.	Azimuth.	Cases per cent.	Azimuth.	Cases per cent.	Azimuth.	Cases per cent.
0° to 10	7	30 to 40°	15	60° to 70°	3	90° to 100°	6	120° to 130°	1	150° to 160°	3
10 to 20	8	40 to 50	10	70 to 80	7	100 to 110	4	130 to 140	3	160 to 170	3
20 to 30	7	50 to 60	9	80 to 90	4	110 to 120	3	140 to 150	3	170 to 180	4

The point towards which the longest diameter is most frequently directed is N. 36° E. If we make a separate comparison of the cases occurring in the Mississippi Valley and those near the

Atlantic coast, we find that the average direction of the longest diameter of the isobars is sensibly the same for both regions.

8. The irregularity in the form of the isobars about a low center appears to be generally due to the unequal force with which the wind on different sides presses inward towards the center of the low area. The depression of the barometer in a low area is considered to be due mainly to the deflecting force arising from the earth's daily rotation upon its axis, as will be more fully explained hereafter.

If the wind pressed in with equal force on all sides towards the low center, and there was no disturbance from local causes, we might expect that the isobars would be exact circles, were it not that the deflecting force arising from the earth's rotation increases with the latitude. It seems, then, well nigh impossible that the isobars should ever be exact circles. The magnitude of the average difference between the greatest and least diameters of the isobars, together with the marked preference which the longer diameter shows for a particular position in azimuth, indicates that the form of the isobars is not wholly determined by an accidental difference between the velocities of the wind on the different sides of the low center.

9. If we examine the cases in which the elongation of the isobars is greatest we may learn something of the causes which produce this elongation, and which determine the position of the longest diameter of the isobars. Plate II exhibits a case of this kind which occurred March 8, 1877, at 7^h 35^m A. M., when a low center was situated between two centers of high pressure not very remote from each other. In cases of this kind, we generally find that the gradients are much the steepest in the direction of the high centers, and hence there results a compression of the isobars in the direction of a line joining the high centers, and an elongation of the isobars in a direction perpendicular to this line. Cases of this kind are of common occurrence in the United States. A low area is almost invariably followed by a high area, which is generally situated on its northwest side; and the low area is generally preceded by a high area on its east or southeast side. Such was the case in the storms represented on Plates I and II. This position of the low center with reference to the high areas causes the longest diameters of the isobars to incline in a direction somewhat east of north.

10. Over the Atlantic Ocean and also over Europe, areas of low pressure resemble the low areas of the United States in their main features, but exhibit several points of difference which ordinarily are pretty clearly marked. Plate III exhibits the isobars for a storm which prevailed over the Atlantic Ocean February 5, 1870. The least diameter of this low area was 2,380 English miles, and the longest diameter was probably about 3,000 English miles. At the center of this low area the barometer stood at 27.33 inches, and the gradient, where steepest, amounted to 0.74 inch for one degree, and on the southeast side of the low center the average gradient up to the isobar of 30 inches was 0.25 inch, and this is nearly double the gradient shown on Plate I. If we compare Plate III with Plate I we perceive that in the former the isobars approach nearest to the figure of a circle; the low area has greater dimensions; the depression of the barometer at the center is greatest; and the barometric gradient is the steepest. In each of these four particulars we generally find a well-marked difference between the low areas of the Atlantic Ocean and those of the United States. This difference appears from a comparison of Hoffmeyer's weather charts (1874–1876) with those of the United States Signal Service. From a comparison of the isobars on Hoffmeyer's charts during a period of three years, I have obtained the following results. The average ratio of the longest diameter of the isobars to the shortest is 1.70:

In 54	per cent. of the whole number of cases,	1.5
17	the ratio of the longest diameter to	2
1	the shortest was greater than	3

If we compare these results with those already given for the United States, we perceive a marked deficiency of very elongated low areas over the Atlantic Ocean.

11. The longest diameter of the isobars over the Atlantic Ocean may be turned in any azimuth, but (as in the United States) it is most frequently directed towards a point somewhat east of north. The following table shows the number of cases in a hundred in which the longest diameter of the

isobars was directed towards each of the ten-degree intervals of azimuth, counting from the north point around by east towards the south:

Azimuth.	Cases per cent.	Azimuth.	Cases per cent.	Azimuth.	Cases per cent.	Azimuth.	Cases per cent.	Azimuth.	Cases per cent.	Azimuth.	Cases per cent.
0° to 10	8	30° to 40°	8	60° to 70°	5	90° to 100°	3	120° to 130°	4	150° to 160°	2
10 to 20	7	40 to 50	9	70 to 80	6	100 to 110	5	130 to 140	6	160 to 170	2
20 to 30	7	50 to 60	8	80 to 90	6	110 to 120	2	140 to 150	4	170 to 180	8

We see that over the Atlantic Ocean the directions of the longest diameters of the isobars are somewhat more equally distributed in azimuth than they are in the United States; nevertheless the quadrant from 80° to 170° of azimuth contains only one third of the whole number of cases, and the center of the region of greatest frequency is N. 35° E., which corresponds almost exactly with the direction already found for the United States.

12. If we examine the cases in which the elongation of the isobars is greatest we shall find that the position of the longest diameter of the isobars is intimately connected with the position of the neighboring areas of high pressure. Plate IV shows the isobars for May 31, 1875, over the Atlantic Ocean and Northern Europe. This plate is copied from the series of Danish weather maps which were issued by Captain Hoffmeyer from December, 1873, to November, 1876, and which are now continued under the joint supervision of the Danish Meteorological Institute and the Hamburg Meteorological Observatory. The isobars represent the atmospheric pressure in millimeters, and are drawn at intervals of 5^{mm}. The barometric observations are all reduced to sea-level, and to the temperature of zero on the centigrade thermometer. Isobars less than 760^{mm} are represented by broken lines; isobars of 760^{mm} and upwards are represented by continuous lines. The direction of the wind is indicated by arrows, and its force is indicated by the number of feathers on the tail of the arrow, according to a scale of 1 to 6 (1 representing the feeblest wind and 6 the strongest).

We find on this plate an area of low pressure stretching from SW. to NE. over a distance of 4,000 miles, and having a breadth from NW. to SE. of 900 miles. The lowest isobar is 740^{mm} (29.13 inches). On the north side of this low area is an area of high pressure (highest isobar shown on the map being 770^{mm} or 30.32 inches). On the south side is also an area of high pressure (highest isobar 765^{mm} or 30.12 inches), and on the east side is a third area of high pressure (highest isobar shown on the map being 770^{mm} or 30.32 inches). The situation of these high areas with reference to the low area is somewhat similar to that represented on Plate II.

13. Within the tropics we occasionally find areas of low pressure in which the winds are very violent and the gradients are very steep, but the geographical extent of the low area is much less than in the great storms of the middle latitudes. Plate V shows the isobars during a violent storm which passed over the Philippine Islands (Asiatic Archipelago) November 5, 1882. On the north and south sides the gradient amounted to 9^{mm} for a half degree, which is at the rate of 18^{mm} (=0.71 inch) for one degree; and this is equal to the steepest gradient shown on Plate III. The greatest velocity of the wind reported was 45 meters per second, or 100 miles per hour, which is greater than any wind reported during the storm represented on Plate III; yet the diameter of this low area, measured from north to south, did not much exceed 500 miles. The cyclones of the tropics frequently exhibit a violence greater than is ever known in the storms of the middle latitudes, but their geographical extent is comparatively small. It will be noticed that on Plate V the winds all incline inward, as on Plates I and III, and show a tendency to circulate about the low center from right to left, but the inclination inward is more strongly marked than in most storms of the middle latitudes.

14. The lower portion of Plate V exhibits the changes of barometric pressure, and also the changes in the direction and force of the wind as shown by self-registering instruments at Manila during the progress of the storm, this place being situated very near the path of the center of low pressure. The pressure in millimeters is indicated on the left margin of the plate; the velocity of the wind is shown on the left margin in meters per second; the hours are shown at the top of the

chart from November 4, 5 A. M., to November 6, 9 A. M.; the direction of the wind for each hour is shown at the bottom of the chart; and the rain-fall is shown on the lower part of the chart as measured at intervals of three hours. The temperature is shown in centigrade degrees on the right margin of the chart, and also the relative humidity.

15. The term *low*, as used in the preceding pages, is to be understood in a relative sense, and does not necessarily indicate that the barometer is below its mean height. The characteristic feature of an area of low pressure is a general movement of the winds inward, and at the same time circulating from right to left about the low center. Such a movement of the winds is found to prevail in the violent cyclones of the West India Islands, and such a system of winds is called a cyclonic system, or simply a cyclone; and an area of low barometer over which such a system of winds prevails, is called an area of cyclonic winds, or simply a cyclonic area. The barometer at the center of such an area may stand as high as 30 inches, and occasionally it stands as high as 30.1, or 30.2, or even higher. Plate VI shows the isobars for the United States on the morning of January 5, 1882, at which time the barometer stood above 30 inches over nearly the whole of the United States, with an area of high pressure (30.8 inches) over the river Saint Lawrence. Near latitude 40° the isobars 30.4 and 30.5 were separated by an interval of over 800 miles, and between them was an area nearly 400 miles in diameter, within which the pressure was less than 30.4 inches.

On the morning of January 4 there was an area of low pressure over Arkansas (the pressure at the center being a little below 30 inches) and it was surrounded by a distinctly marked system of circulating winds. During the next twenty-four hours this low area advanced about 450 miles toward the northeast, and during this time the barometer had been continually rising, and the system of circulating winds was generally supplanted by feeble winds from some northern quarter. At only one station on the morning of January 5 was the wind within this area reported from the south. The isobar of 30.4 inches included a region over which the pressure was lower than the pressure immediately surrounding it, that is, the pressure was relatively low, but there remained only a slight trace of the system of circulating winds which had previously prevailed. The term *cyclonic* area, when applied to a system of circulating winds with pressure above 30 inches, is more descriptive than the term *low* area; but both of these terms are in common use.

16. A comparison of Plates I to VI shows that we may have deep cyclones as seen in Plates I and III, or shallow cyclones as seen in Plate VI, and there is a corresponding difference in the velocity of the winds in the two cases. The winds shown in Plate I were very strong, particularly on the northwest side of the low center, being 48 miles an hour at Dodge City, 36 miles at Yankton, 32 miles at Leavenworth, and 31 miles at Escanaba. The winds represented on Plate III were still more violent, five vessels having reported the force of the wind as rising to 10 on Beaufort's scale, which is considered to be equivalent to a velocity of 65 miles per hour; and one vessel reported the force of the wind as 12 on the same scale, indicating a velocity of 90 miles per hour. Within the low area represented on Plate VI the highest wind reported was at Nashville, 6 miles per hour, while at Columbus and Louisville the velocity was 4 miles per hour; at Cincinnati and Indianapolis, 3 miles per hour; and at Knoxville only 1 mile per hour.

17. When an area of low pressure is very much elongated we frequently find two cyclonic centers included within the same area of low pressure. Plates I, II, and III show only one low center, but Plate IV shows two centers of cyclonic movement within the same area of low pressure, besides three less important centers on the lower part of the chart, and each of them is attended by a system of feeble winds circulating about it. When there are two cyclonic centers within the same area of low pressure these centers are generally of unequal depth; but sometimes they are

	Miles per hour.		Miles per hour.		Miles per hour.
Sandy Hook, N. J.	74	Buffalo, N. Y.	36	Kitty Hawk, N. C.	32
Cape May, N. J.	70	Cleveland, Ohio	36	Morgantown, W. Va.	32
Erie, Pa.	48	Albany, N. Y.	35	Alpena, Mich.	30
Barnegat, N. J.	40	New York City	34	Atlantic City, N. J.	30
Cape Lookout, N. C.	38	Cape Henry, Va.	33	Norfolk, Va.	30

sensibly equal, as shown in Plate VII, which represents the isobars over the eastern part of the United States on the morning of December 9, 1876. Here we notice a center of high pressure (30.4 inches) on the western side of the low area, and on that side of the low area the gradients are very steep, and the wind velocities are very high, as shown in the preceding table.

This table shows the wind velocities reported at $7^h\ 35^m$ A. M., but these were not in all cases the highest velocities reported during the progress of this storm. The following table shows the highest velocities reported:

	Miles per hour.		Miles per hour.		Miles per hour.
Sandy Hook, N. J	84	Boston, Mass	50	Philadelphia, Pa	42
Cape May, N. J	72	Wood's Holl, Mass	50	New Haven, Conn	40
Newport, R. I	60	Grand Haven, Mich	49	Portland, Me	38
New York City	60	Erie, Pa	48	Rochester, N. Y	36
Marquette, Mich	54	Barnegat, N. J	45	Oswego, N. Y	36
Cape Lookout, N. C	50	Eastport, Me	43	Port Huron, Mich	35

The winds reported in the neighborhood of New York City are the highest winds that I have found reported at any of the Signal Service stations since the commencement of the observations in 1872, with the exception of Mount Washington.

Over New England the isobars are very much elongated in a direction nearly perpendicular to a line joining the high and low centers. Within the isobar 29.2 we find two isobars of 29.1; and at the most northerly stations the winds appear to be mainly controlled by the northern center, and at the more southerly stations the winds are mainly controlled by the southern center. Between two neighboring centers of low pressure we generally find the directions of the wind to be irregular, at certain places being mainly controlled by one of the centers, and at other places being mainly controlled by the other center.

18. Sometimes within a large area of low pressure we find three centers of cyclonic movement of the winds. These cyclonic centers are generally of unequal depth, but occasionally we find them sensibly equal. Plate VIII shows the isobars over the Atlantic Ocean and Europe on the morning of March 12, 1876. This plate is constructed on the same plan as Plate IV. Here we see three low centers, and the lowest isobar about each of them is 730^{mm}, and we notice a high area (770^{mm}) on the west side, another high area (770^{mm}) on the southwest side, and a third high area (770^{mm}) on the northeast side. The winds over a large portion of this low area are extremely violent, particularly on the south and west sides of the low center which prevails over England, where the winds rise to 6 (on a scale of 1 to 6); and on the northeast side of this center, to a distance of about 350 miles, the winds appear to be controlled by this center. A little further to the northeast the winds are controlled by the low center over Sweden, but on the north side of the Swedish low center the winds are generally feeble, and are mainly controlled by the third low center on the northwest of Norway.

19. Occasionally, within a large area of low pressure, we find four or five or even more cyclonic centers, and when the number of centers is so great it seldom occurs that they are all of equal depth. Plate IX shows the isobars over Europe and the Atlantic Ocean, on the morning of March 9, 1876. The principal center of low pressure (715^{mm}) is north of Scotland, and about this center the winds are very violent, rising to number 6 on a scale 1 to 6, and the gradients are steep, particularly on the western side. The cyclonic motion of the winds is strongly marked, the circulation of the winds about the low center being very decided, while the inward tendency is not as great as is generally found in cases where the winds are less violent. On the eastern side of this principal cyclonic center the winds are more feeble, and here we find four minor centers of cyclonic movement. Near St. Petersburg is a low center (740^{mm}), about which the cyclonic movement of the winds is distinctly marked. Near the parallel of 50° is a third low center (745^{mm}), where the winds are generally feeble, but they show considerable cyclonic tendency. South of the Black Sea is a fourth low center (750^{mm}), where the observations are few, but those which are represented on the chart show a distinct cyclonic tendency; while over the Caspian Sea is a fifth

low center (755^mm). On the northwest side of this immense area of low pressure is an area of high pressure (775^mm), and on the southwest side is another area of high pressure (775^mm). On the east side the highest isobar represented is 765^mm, but the length of this isobar and its relation to the low pressure on the western side lead us to expect higher pressure further east, and by consulting observations in Asiatic Russia (not represented on the map) we find that the pressure continued to increase in advancing eastward.

20. Sometimes we find areas of low pressure of greater extent than any of the preceding, and showing numerous centers about which the winds circulate with considerable force, when the barometric depression is considerable, but feeble when the depression is small. The international weather maps show numerous examples of this kind. According to these maps, on the morning of June 7, 1882, there was an area of low pressure which covered the whole of Asia, and apparently extended from the equator to a considerable distance beyond the North Pole; it covered the whole of Europe with the exception of a very small portion of its southern margin; it covered the northern part of the Atlantic Ocean, and reached across the central portion of North America to the Pacific Ocean, extending thus through about 320 degrees of longitude. The principal low center (29.2 inches) was north of the Caspian Sea; a second low center (29.4 inches) was over the northern part of India; a third low center (29.6 inches) over the Gulf of Saint Lawrence; a fourth low center (29.8 inches) over China; a fifth low center (29.8 inches) northeast of Japan; and if the observations were sufficiently numerous, there is little doubt that several other subordinate low centers would be exhibited. A center of high pressure (30.4 inches) was found over the Atlantic Ocean near latitude 35°; a second center of high pressure (30.2 inches) over the southeastern part of the United States; and a third center (30.2 inches) over the eastern part of the Pacific Ocean near latitude 30°. The area of high pressure formed a belt following the parallel of 30° or 35°, and extending through at least 240° of longitude, but interrupted by the Asiatic Continent.

21. Plate IX illustrates the tendency to the formation of subordinate centers of cyclonic action, whenever within a very extensive area of low pressure the winds are comparatively feeble. When this tendency is slight it simply causes a little distortion in the isobars without the formation of a distinct area of cyclonic action. There are few cases of great storms in which we do not find some distortion of the isobars which may be ascribed to this cause. In Plate III most of the isobars are uncommonly symmetrical, but we notice a distortion of the isobar 30.0 inches over Spain, and the winds in this neighborhood indicate a feeble center of cyclonic action. The same remark is illustrated by Plates IV and VIII.

22. From an examination of the Signal Service maps, we find that in the United States an area of low barometer, with only one system of cyclonic winds, frequently has a diameter of 1,600 English miles. From Hoffmeyer's charts we find that over the Atlantic Ocean such an area frequently has a diameter of 2,000 English miles. Areas of low barometer, having several centers of cyclonic action, may have a diameter of 6,000 English miles, and may form a belt extending nearly (if not entirely) around the globe, between the parallels of 40 and 50 degrees.

Direction of movement of areas of low pressure.

23. Areas of low pressure seldom remain stationary in position for many hours. The center of low pressure generally changes its position steadily from hour to hour, and everywhere we find a marked uniformity in the direction of this movement. Plate X shows the tracks of a large number of centers of low pressure, for the United States and the adjacent districts. This plate is not designed to indicate the track which storm centers most frequently pursue, but rather to give an example of all the more important tracks pursued by storm centers, as delineated on the Signal Service maps. The tracks represented on the northern part of the chart are those which most frequently occur, while those on the southern part of the chart are comparatively infrequent. We perceive that north of the parallel of 30° storms generally travel from west towards the east; but in some places they deviate to the south of east, and in other places they deviate to the north of east. On the southeast portion of the chart we notice several tracks which are directed towards the northwest.

24. A chart which represents storm tracks for the entire northern hemisphere is best adapted

to suggest the cause of these movements. Plate XI affords an example of nearly all the different storm tracks delineated on the international maps of the United States Signal Service for a period of more than four years. We perceive that north of the parallel of 30° storm tracks in all longitudes almost invariably pursue an easterly course, but generally they show an inclination towards the north of east; while within the tropics storm tracks almost invariably tend westerly, with an inclination towards the north of west. We also notice that none of the storm tracks delineated on the chart reaches down to the equator. The lowest latitude of any center of low pressure which has been distinctly traced is 6.1° N., and there are eight cases of cyclonic storms whose paths have been traced to points south of latitude 10° N.

25. It is not, however, to be understood that at the equator the wind does not sometimes rise to the force of a gale, but rather that a regular system of cyclonic winds, with a considerable depression of the barometer, has never been known to prevail directly under the equator. Hard gales and violent squalls of wind do however sometimes occur directly under the equator. This is shown by various logs quoted in Piddington's Memoirs. The following is an example from the log-book of the Winifred, quoted in Piddington's Eleventh Memoir, pages 30 to 40:

	Lat.	Long.	Bar.	Wind.	
1843.					
Nov. 26, noon.	9° 40′ N.	85° 48′ E.	29.80	E.	Strong squalls and heavy rain.
27, "	7 4 N.	85 56	29.67	ENE.	Sudden and dangerous gusts and violent squalls.
28, "	4 27 N.	85 58	29.65	NW.	Most terrific squalls. Reduced sail to double reef topsail.
29, "	1 20 N.	86 30	29.59	NNW.	Succession of dangerous squalls.
30, "	1 1 S.	86 0	29.64	W.	Violent and terrific squalls.
Dec. 1, "	3 15 S.	86 56	29.67	NW.	Violent varying squalls.
2, "	4 21 S.	87 34	29.74	....	Calm weather.

The following is from the log-book of the Fyzul Curreem for the same period:

	Lat.	Long.	Wind.	
1843.				
Nov. 27, noon.	5° 11′ N.	83° 36′ E.	NNW.	Heavy squalls.
28, "	2 6 N.	83 40	W. by S.	Fresh gale.
29, "	0 54 S.	84 31	W.	Fresh gale, increasing with heavy squalls to a strong gale.
30, "	3 50 S.	85 27	W.	Fresh gales.
Dec. 1, "	5 39 S.	85 37	NNW.	Strong sea from WSW.
2, "	6 41 S.	85 1	NNE.	Heavy head sea.

These observations show that directly under the equator we may have winds of a dangerous violence, accompanied by frequent changes in direction, indicating somewhat imperfectly a cyclonic character, and accompanied by sudden oscillations of the barometer, which are very unusual near the equator. Within six degrees of the equator the depression of the barometer has, however, never been found sufficiently great, and the depression has not been maintained with sufficient steadiness to enable us to identify an area of low pressure in its progress from day to day.

26. Although violent gales do sometimes occur directly under the equator, they are of very rare occurrence. This is shown by Maury's Storm Chart of the North Atlantic Ocean, which gives the number of gales which have been observed on the Atlantic Ocean in different latitudes from the equator as far north as latitude 60°. On this chart the ocean is divided into squares by parallels of latitude drawn at intervals of five degrees from each other, and meridians of longitude at intervals of five degrees. The following table presents a summary of the results of this chart. Each square of the table contains three numbers. The first shows the number of observations within the given square, each observation representing a period of eight hours. The second shows the number of gales reported, and the third shows the average number of gales occurring in a hundred observations. Thus in the square included between the parallels of 40° and 45° of north latitude, and between the meridians of 45° and 50° west longitude from Greenwich, the first number is 1,863,

which shows the number of observations obtained in that square. The second number is 280, which denotes the number of gales reported; the third number is 15, which denotes that the number of gales was 15 per cent. of the whole number of observations.

TABLE I.—*Gales on the Atlantic Ocean by Maury's Storm Chart.*

Lat.	80°–75°	75°–70°	70°–65°	65°–60°	60°–55°	55°–50°	50°–45°	45°–40°	40°–35°	35°–30°	30°–25°	25°–20°	20°–15°	15°–10°	10°–5°	5°–0°
60°–55°											60 16 27	102 38 37	123 35 28	117 31 27	78 12 15	30 3 10
55°–50°								150 57 38	420 111 26	510 140 27	694 169 24	850 159 19	932 117 12	1270 152 12	1393 133 10	583 46 8
50°–45°					126 11 9	288 28 10	919 121 13	1242 209 17	1570 369 24	1740 277 16	1627 242 15	1539 165 11	1478 160 11	1312 156 12	920 85 9	313 13 4
45°–40°		1820 126 7	3249 260 8	2544 266 11	2679 269 10	2419 241 10	1863 280 15	1581 234 15	1119 127 11	732 66 9	396 58 15	269 44 16	168 16 9	128 9 8	67 1 1	0
40°–35°	243 8 4	4193 607 14	2974 475 16	1797 393 22	1393 177 13	1100 115 10	773 62 8	480 28 6	349 27 8	242 23 9	341 35 10	268 5 2	302 24 8	340 7 2	334 9 3	225 14 6
35°–30°	1534 126 8	2265 231 10	1645 137 8	766 65 9	723 72 10	860 71 8	986 60 6	893 48 5	747 27 3	392 25 6	175 4 2	129 0 0	223 9 4	77 0 0	3 0 0	
30°–25°	1945 81 4	1393 30 2	1137 57 5	948 17 2	394 17 4	351 6 2	564 5 1	726 9 1	958 61 6	663 6 1	209 8 4	153 0 0	87 6 7			
25°–20°	316 12 4	380 9 2	262 4 2	650 7 1	637 6 1	267 6 2	262 9 3	452 18 1	806 4 0	664 25 4	338 0 0	136 0 0	15 0 0			
20°–15°	243 4 2	320 2 1	152 0 0	183 0 0	459 1 0	541 4 1	326 1 0	302 10 3	449 6 1	711 13 2	638 8 1	159 0 0	6 0 0			
15°–10°	65 0 0	0 0 0	53 1 2	53 0 0	96 1 1	387 0 0	594 1 0	508 0 0	415 3 1	667 9 1	835 21 3	622 0 0	225 0 0			
10°–5°					23 0 0	0 0 0	289 0 0	668 0 0	632 0 0	739 3 0	1667 5 0	1109 0 0	483 0 0	716 0 0	80 0 0	70 0 0
5°–0°							95 0 0	421 0 0	613 0 0	1004 0 0	2004 3 0	1262 0 0	335 0 0	107 0 0	362 1 0	233 0 0

The following table presents a summary of the results for all parts of the Atlantic Ocean for each five degrees of latitude:

TABLE II.—*Summary.*

Latitude.	Obs.	Gales.	Ratio.	Latitude.	Obs.	Gales.	Ratio.	Latitude.	Obs.	Gales.	Ratio.
Equator to 5° N.	6,436	1	.0006	20° N. to 25° N.	5,185	100	.0193	40° N. to 45° N.	19,034	1,997	.1049
5 N. to 10 N.	6,476	8	.0012	25 " to 30	9,528	303	.0318	45 " to 50	13,074	1,836	.1404
10 N. to 15 N.	4,520	36	.0080	30 " to 35	11,118	875	.0766	50 " to 55	6,792	1,084	.1596
15 N. to 20 N.	4,489	19	.0109	35 " to 40	15,354	2,009	.1308	55 " to 60	510	135	.2647

From this table we see that on the Atlantic Ocean, between the equator and latitude 5° N., gales occur on an average somewhat less frequently than once a year; and from latitude 5° to

latitude 10° N. they occur a little more frequently than once a year. From latitude 10° N. to latitude 15° N. there occur on an average nine gales annually; and as we advance northward the frequency of gales increases with the latitude up to latitude 60° N.

27. On Maury's storm chart for the eastern half of the North Pacific Ocean among 17,854 observations between the equator and 5° N. latitude, 35 gales are reported; and between 5° and 10° N. latitude among 9,352 observations, 33 gales are reported. These observations indicate that in the low latitudes of the Pacific Ocean gales are somewhat more frequent than over the Atlantic Ocean. It appears evident that in both oceans, within 6° of the equator, gales are of extremely rare occurrence, and when they do occur the depression of the barometer is small, and the cyclonic character of the winds is indistinctly marked. North of the parallel of 6° the cyclonic character of the winds becomes more distinct, and areas of low pressure can be identified in their progress from day to day.

28. The tropical cyclones which have been found to pursue a westerly course are limited to two districts: 1. The Atlantic Ocean, and chiefly its western part near the West India Islands. 2. The region south of the continent of Asia. Tropical cyclones have never been observed in any part of the Pacific Ocean, with the exception of its western portion near the continent of Asia and the neighboring islands. I have made a careful study of the cyclones of each of these localities. Table III exhibits some of the particulars respecting each of the cyclones originating near the

TABLE III.—*Course of cyclones originating near the West India Islands.*

No.	Date.	Latitude of beginning.	Course while moving westward.	Velocity in miles per hour.	Latitude when moving north.	Course while moving eastward.	Velocity in miles per hour.	Rain-fall	Investigators.	Where recorded.
1	1780, Oct. 3	16.5				E. 61.5 N.		Hard rain	Reid	Law of Storms, p. 273.
2	1780, Oct. 12	11.8	W. 31 N.	17.8	23.3	E. 39.5 N.	17.2	Hard rain	Reid	Law of Storms, p. 273.
3	1804, Sept. 3	15.7	W. 30 N.	20.4	31.2	E. 46 N.	18.1	V. hard rain	Redfield	Jo. Sci., v. 20, p. 17.
4	1821, Sept. 1	21.7	W. 27 N.	35.0	31.2	E. 55 N.	25.6	Hard rain	Redfield	Jo. Sci., v. 20, p. 17.
5	1827, Aug. 17	14.8	W. 29 N.	12.9	30.0	E. 43 N.	10.0	Hard rain	Redfield	Jo. Sci., v. 31, p. 123.
6	1830, Aug. 12	17.3	W. 23.5 N.	23.8	31.4	E. 37 N.	16.3	V. hard rain.	Redfield	Jo. Sci., v. 20, p. 34.
7	1830, Aug. 22	22.3	W. 27 N.	18.7	30.3	E. 40 N.	16.0	Rain	Redfield	Jo. Sci., v. 20, p. 39.
8	1830, Sept. 29	20.2	W. 33.5 N.	26.4	30.4	E. 43 N.	29.6		Redfield	Jo. Sci., v. 20, p. 42.
9	1831, Jan. 13	30.0			30.6	E. 53.5 N.	16.6	Snow	Redfield	U. S. Naval Mag. 1836.
10	1831, June 23	10.3	W. 14.5 N.	20.4					Redfield	Jo. Sci., v. 31, p. 123.
11	1831, Aug. 10	12.3	W. 25.5 N.	16.6	30.7			Hard rain	Redfield	Jo. Sci., v. 21, p. 192.
12	1835, Aug. 12	16.3	W. 17 N.	17.8				Rain	Redfield	Jo. Sci., v. 31, p. 124.
13	1835, Sept. 3	12.4	W. 38 N.					Hard rain	Reid	Law of Storms, p. 36.
14	1837, July 26	11.0	W. 29 N.		30.0			V. hard rain.	Reid	Law of Storms, p. 48.
15	1837, Aug. 2	17.3	W. 31.5 N.					Hard rain	Reid	Law of Storms, p. 48.
16	1837, Aug. 12	17.6	W. 20 N.		31.7	E. 24.5 N.		Rain	Reid	Law of Storms, p. 69.
17	1837, Aug. 24	32.7				E. 47 N.	17.5	Hard rain	Reid	Law of Storms, p. 109.
18	1837, Sept. 27	15.7	W. 24 N.	8.3	26.2	E. 17.5 N.	13.4	Hard rain	Reid	Progress, p. 13.
19	1839, Sept. 12	18.5	W. 26 N.		32.2			Hard rain	Reid	Progress, p. 39.
20	1842, Aug. 30	21.6	W. 1 N.	10.0				Rain	Redfield	Jo. Sci., v. 1, p. 2.
21	1842, Oct. 2	20.0				E. 18 N.	10.6	Hard rain	Redfield	Jo. Sci., v. 1, p. 153.
22	1844, Oct. 4	18.6				E. 54 N.	30.4	Hard rain	Redfield	Jo. Sci., v. 2, p. 312.
23	1846, Sept. 11	13.8	W. 62 N.	10.3	29.2	E. 47 N.	14.3	Hard rain	Redfield	Jo. Sci., v. 18, chart.
24	1846, Oct. 6	14.2	W. 60 N.		30.0	E. 60.5 N.	23.5	Rain	Redfield	Jo. Sci., v. 18, chart.
25	1847, Oct. 10	12.8	W. 11.5 S.	21.2				Rain	Reid	Progress, chart.
26	1848, Aug. 22	15.0	W. 28.5 N.		27.4	E. 22 N.			Reid	Progress, p. 337.
27	1848, Aug. 29	15.0	W. 22 N.		29.0	E. 24 N.		Hard rain	Maury	Phys. Geog., p. 60.
28	1850, Sept. 2	16.0	W. 5 N.	13.8				Rain	Redfield	Jo. Sci., v. 18, p. 176.
29	1851, Aug. 16	13.5	W. 15 N.	17.5	27.3	E. 34 N.	18.7	Rain	Redfield	Jo. Sci., v. 18, chart.
30	1853, Aug. 30	12.5	W. 12.5 N.	25.3	31.7	E. 24.5 N.	28.4	V. hard rain	Redfield	Jo. Sci., v. 18, p. 1.
31	1853, Sept. 26	28.8			29.2	E. 27 N.		V. hard rain.	Redfield	Jo. Sci., v. 18, p. 180.
32	1853, Sept. 29	13.9	W. 9 N.					Hard rain	Redfield	Jo. Sci., v. 18, p. 178.
33	1866, Oct. 1	19.0	W. 15 N.	15.0	26.1	E. 25 N.	30.0	Rain	Buchan	Handy Book, p. 151.
34	1867, Oct. 29	18.[illegible]	W. 2 S.	15.5				V. hard rain	Eastman	Pamphlet.
35	1871, June 1	23.5	W. 44 N.	12.3	31.5	E. 15 N.	23.5	Rain	Sig. Serv.	Report 1872, p. 282.
36	1871, Sept. 5					E. 38 N.	15.0	Rain	Sig. Serv.	Report 1874, map.
37	1873, Aug. 18	20.0	W. 32 N.	12.3	33.0	E. 37 N.	18.4	Rain	Sig. Serv.	Report 1873, p. 1029.
38	1873, Aug. 20	25.0	W. [illegible] N.	10.5	31.3	E. 41 N.	16.4	V. hard rain.	Toynbee	Jo. Met. Soc., v. 2, p. 15.
39	1873, Oct. 6	21.3	W. 28 N.	9.5	21.3	E. 45 N.	30.1	Hard rain	Sig. Serv.	Monthly Map, 1873.
40	1874, Feb. 7	24.0			26.5	E. 45 N.	23.5	Rain	Sig. Serv.	Report 1874, map.
41	1875, Sept. 14	23.0	W. 22 N.	25.1	28.5	E. 24 N.	29.6	Hard rain	Sig. Serv.	Monthly Map, 1875.

West India Islands, and for which definite paths have been determined. Column 1 shows the number of reference; column 2 gives the date of commencement of the storm so far as ascertained; column 3 shows the latitude of the storm's center, when it first became violent; column 4 shows the average course of the storm while moving westward; column 5 shows the hourly velocity of progress in the preceding part of its course; column 6 shows the latitude at which the storm was moving due north; column 7 shows the average course of the storm after turning eastward, until it reached the parallel of 40°; column 8 shows the hourly velocity of progress during the preceding period; column 9 shows whether rain was mentioned as accompanying the storm; column 10 gives the name of the person by whom the phenomena of the storm were investigated, and column 11 shows where the record of the investigation may be found.

29. It will be noticed that the least latitude of any storm path here recorded is 10°; that is, over the Atlantic Ocean no storm path has been traced within 10° of the equator.

The courses of the storms mentioned in this table (while moving westward) range from 11½° south of west to 62° north of west. In two cases the course was a little south of west; in a third case the course was only one degree north of west, and in a fourth case the course was only five degrees north of west. Tropical storms do therefore sometimes travel towards the equator, and it may be suspected that this direction occurs more frequently than the table would indicate, since many of the storms here recorded would never have been selected for investigation if they had not advanced into the middle latitudes. The average course of the storms here enumerated, while they were moving westward, was west 26° north; and the average hourly velocity in this part of their course was 17.4 miles.

The average latitude of the storm's center when moving due north was 29½°, and the latitudes range from 23¼° to 34°. During the three summer months the average latitude is 30°.6; in September it is 29°.7, and during the other months of the year 26°.7, indicating that the point where the course changes from west to east is somewhat more northerly in summer than in winter. The average course of these storms while traveling eastward to the parallel of 40° was E. 38½° N., ranging from 17° to 60°. The average hourly velocity in this part of their course is 20.5 miles, which is a little less than the average velocity of storms in the United States for the months of August and September, according to the Signal Service observations. It will be seen from column 9, that rain generally accompanies cyclones. In three of the cases I have found in the published reports no mention of rain, but it is presumed that this is simply an oversight, since in most of the other cases rain is only incidentally mentioned. In all the investigations of Redfield and Reid the circumstances upon which they insist as specially important are the direction and force of the wind, and it is only by consulting the extracts from the log books which they have furnished us that I have discovered any mention of accompanying rain. It is believed that tropical cyclones *never* occur without rain, and generally the rain is described as descending in *torrents*. The letter V, in column 9, signifies *very*.

30. In order to obtain more complete information respecting the tracks of tropical cyclones in the neighborhood of the West India Islands, I have compared all the storm tracks delineated on the maps of the Monthly Weather Review, and also those delineated on the international charts.

The following table shows the leading particulars respecting those storms whose course was for some days towards the west:

TABLE IV.—*American storms advancing westerly.*

No.	Date.	Latitude. Beg. End.	Longitude. Beg. End.	Course.	Velocity, miles.	Subsequent course.
1	1873, June 1.1– 2.3	24°–32°	80°–86°	NNW.	12.5	Became extinct.
2	Oct. 2 – 4.2	22–24	82– 86	NW.	9.5	Moved NE.
3	1874, Feb. 7 – 8.1	24–27	82– 83	NNW.	15.4	Moved NE.
4	July 2.3– 4.2	27–29	87– 98	WNW.	13.1	Became extinct.
5	Sept. 4.3– 5.3	25–32	65– 70	NNW.	22.5	Moved NE.
6	1875, Sept. 8.3–17.1	14–29	59– 96	WNW.	13.2	Moved NE.
7	1876, Sept. 15 –18.1	21–43	69– 80	NNW.	25.9	Moved E.
8	1877, Sept. 22.2–30.3	12–26	65– 88	WNW.	11.1	Moved NE.
9	1878, Aug. 12 –18	14–21	75– 97	WNW	14.4	Unknown.
10	Sept. 1 – 8	11–28	59– 81	WNW.	9.3	Moved N.
11	Sept. 12 –18	14–29	47– 60	NW.	9.6	Moved NE.
12	Sept. 24 –30	14–28	70– 73	NNW.	5.3	Moved NE.
13	Sept. 29 –34	22–30	58– 70	NW.	9.1	Moved NE.
14	Oct. 9 –13	15–26	40– 52	NW.	7.2	Moved NE.
15	Oct. 13 –18	17–30	36– 55	NW.	13.2	Moved N.
16	Nov. 25 –30	15–17	52– 73	W.	11.7	Unknown
17	1879, Aug. 13 –17	18–30	60– 77	NW.	8.2	Moved NE.
18	Aug. 15 –16	14–14	43– 51	W.	(?)	Unknown.
19	Aug. 20 –23	16–29	87– 94	NW.	8.2	Moved E.
20	Oct. 3 – 7	15–31	78– 90	NW.	8.1	Became extinct.
21	Oct. 10 –17	14–43	70– 90	NW.	11.1	Moved E.
22	1880, Aug. 6 –14.2	12–32	77–103	WNW.	12.9	Disappeared.
23	Aug. 15 –19	13–20	62– 78	WNW.	12.0	Moved NE.
24	Aug. 24 –31	26–33	60– 89	WNW.	10.0	Disappeared.

Column 1 shows the number of reference; column 2 shows the dates of beginning and end of the observed movement as long as the course continued westerly; column 3 shows the latitude at the beginning and end of this portion of the path; column 4 shows the longitude at the beginning and end of this portion of the path; column 5 shows the prevalent direction of the path while moving westerly; column 6 shows the average velocity of progress of the storm center (in miles per hour) while the course continued westerly; column 7 gives a brief indication of the subsequent course of each storm. On Plate XII, Fig. 1, these tracks are delineated, and are designated by the same numbers as in the table.

31. The general results of this table correspond very closely with those deduced from Table III. The lowest latitude of any storm center shown in this table is 10°.6 N. The lowest latitude shown in Table III is 10°.3 N. The average velocity of these storms while moving westerly was 11.9 English statute miles per hour; the average velocity of the storms mentioned in Table III while moving westerly was 17.4 miles per hour. In nine of these cases the course of the storm became due north before reaching the parallel of 30°, and the average direction of these storms in the early part of their course was west 20°.5 north.

Storm No. 18 apparently advanced directly west, and storms Nos. 9 and 16 apparently moved for a day or two a little south of west. Table III shows thirty one cases in which the course of storms was towards the north of west, and only two cases in which the course was south of west, viz. one case in which the course was two degrees south of west, and the other eleven degrees south of west. From the two tables we perceive that the cases in which tropical storms move in a direction north of west are fifteen times as frequent as the cases in which they move in a direction south of west, and in none of the cases here reported was the southerly motion very decided.

32. In order to determine whether during the period here considered there may not possibly have been other storms which moved in a direction corresponding more nearly with that of the northeast trade winds, I have made a careful comparison of the international observations. Five sixths of all the storms enumerated in Table IV occurred in the months of August, September, and October. I therefore selected these three months for special comparison. For the years 1876–'77–'78 and '79 the barometric curves were drawn for these months for all the stations reported in the International Bulletin, between the equator and latitude 26° N.

An examination of these curves shows that at all of these stations the fluctuations of the barometer were very small, particularly for the stations nearest to the equator. At Paramaribo, latitude 5° 45′ N., the entire range of the barometer for these twelve months was only 0.20 inch, and there was no oscilliation which can be identified with an oscillation at either of the other stations. At Bridgetown, latitude 13° 4′ N., the entire range of the barometer for these twelve months was 0.23 inch. Two or three of the barometric oscillations at this station can probably be identified with oscillations at some of the other stations. The track of storm No. 9 can apparently be traced back to Bridgetown on the 10th of August, 1878. At Fort de France, latitude 14° 40′ N., the entire range of the barometer for these twelve months was 0.42 inch, and six or seven of the barometric oscillations at this station can probably be identified with oscillations at some of the other stations.

Besides the areas of low barometer enumerated in Table IV there are but few others during this period which can be traced with confidence from one station to another. In 1876 the number of stations of observation in the tropical regions was small, and the storm of September 15–18 is the only one which can be satisfactorily traced from these observations.

In 1877 the center of storm No. 8 passed at a considerable distance from all of the reporting stations, and is only obscurely indicated by the published observations. On the 26th of August a small but well-marked barometric depression occurred almost simultaneously at all of the stations from Fort de France to Havana. On the 17th and 18th of October there was a noticeable fall of the barometer, which apparently advanced from San Juan de Porto Rico to Havana.

In 1878, from September 15 to 16, a small barometric depression traveled from Bridgetown to Santiago de Cuba. From the 2d to the 3d of October a small barometric depression traveled from Fort de France to Nassau. On the 21st of October there was a decided barometric depression at Vera Cruz and Havana, which advanced northerly along the coast of the United States, and was marked by great violence.

In 1879, from the 16th to the 18th of August, a small barometric depression traveled from Bridgetown to San Juan de Porto Rico. This was, perhaps, a continuation of No. 18 of Table IV, and, if so, it shows that this storm veered a little to the north of west, like most of the storms of this region. On the 28th of August a small barometric depression appeared almost simultaneously at all the stations from Navassa to Tlacotalpam, on the coast of Mexico. This depression apparently advanced northward, but the published observations are not sufficient to enable us to trace its course satisfactorily.

This examination has disclosed a few barometric depressions, in addition to those enumerated in Table IV, but their courses were generally towards the north of west. We therefore seem authorized to conclude that nearly all the areas of low barometer which occur within the tropics, and advance westward in the neighborhood of the West India Islands, instead of following the ordinary course of the trade-winds advance in a direction somewhat north of west.

33. I have endeavored to ascertain what was the prevalent direction of the wind which preceded each of these tropical storms, and also the prevalent wind which succeeded the low center, and how these two winds generally compared in respect of force. It is impossible to make a satisfactory comparison from the observations in the International Bulletin, on account of the small number of stations, and because the observations are reported only once a day. The following tables show the height of the barometer, together with the direction and force of the wind, in the case of four of the low areas enumerated in Table IV, for the stations nearest the center of low pressure. The velocity of the wind in miles per hour is shown by the numbers without parentheses; the numbers in parentheses show the force of the wind estimated on a scale from 1 to 10:

No. 9.—1878. *August* 10–15.

	August 10.	August 11.	August 12.	August 13.	August 14.	August 15.
San Juan	30.04 SE. 2	29.98 E. 12	30.03 SE. 4	30.02 SE. 8	30.03 SE. 6	30.05 SE. 2.
Navassa	29.98 SE. 12	29.99 NE. 10	29.89 N. 19	29.92 SE. 29	29.91 E. 17	29.97 E. 17.
Kingston	30.15 calm	30.17 calm	30.08 calm	30.09 SE. 10	30.09 SE. 20	30.12 calm.
Nassau	30.04 SE. (1)	30.02 NE. (2)	30.03 NE. (2)	29.96 SE. (1)	29.97 SE. (2)	30.05 SE. (2).
Havana	30.00 E. 2	30.02 ESE. 4	29.99 ESE 3.	29.90 ENE. 4	29.81 E. 9	29.90 SE. 16.

No. 10.—1878, *September* 3–8.

	September 3.	September 4.	September 5.	September 6.	September 7.	September 8.
Navassa	29.93 S. 19	29.79 N. 20	29.82 S. 22	29.89 SE. 20	29.94 E. 14	29.93 E. 15.
Santiago de Cuba	29.96 NE. 7	29.85 N. 6	29.68 SE. (6).	29.85 SE. (4)	29.91 SE. 6	29.91 calm.
Kingston	30.10 calm	30.01 calm	29.96 E. 3	30.08 S. 13	30.10 SE. 18	30.11 calm.

No. 12.—1878, *September* 24–29.

	September 24.	September 25.	September 26.	September 27.	September 28.	September 29.
San Juan	29.91 NE. 6	29.87 SE. 11	29.95 SE. 1	29.92 SE. 6	29.91 SE. 4	29.88 SE. 4.
Navassa	29.95 NW. 12	29.85 NNE. 18	29.83 E. 8	29.82 NW. 12	29.70 S. 25	29.78 S. 15.
Santiago de Cuba	29.96 N. (1)	29.88 NNE. 8.	29.85 NNE.(1)	29.79 N. (1)	29.71 NW. (1)	29.72 SW. (1).

No. 21.—1879, *October* 10–15.

	October 10.	October 11.	October 12.	October 13.	October 14.	October 15.
San Juan	29.97 SE. 4	29.89 SE. 7	29.89 SE. 0	29.96 SE. 0	29.99 SE. 0	30.01 SE. 0.
Navassa	29.87 NE. 5	29.82 E. 10	29.79 SE. 16	29.76 E. 20	29.92 SE. 15	29.95 SE. 18.
Santiago de Cuba	29.92 N. 7	29.86 N. 2	29.79 SE. 10	29.83 SE. 6	29.90 SE. 8	29.96 SE. 6.
Kingston	30.11 calm	30.01 calm	29.99 SE. 4	30.04 SE. 18	30.11 SE. 6	30.17 calm.
Nassau	30.02 NE. (3).	29.97 NE. (1).	29.92 NE. (2)	29.91 NE. (3)	29.88 E. (2)	29.95 SE. (1).
Havana	29.98 ENE. 10	29.90 E. 6	29.86 ENE. 8.	29.76 E. 12	29.64 E. 20	29.75 SSE. 18.

It will be seen that at several of these stations the fluctuation of the barometer was small; the winds were feeble, and their cyclonic character was indistinctly marked; but at those stations where the fluctuation of the barometer was greatest, there was a decided change in the direction of the wind about the time of least pressure. In two-thirds of the cases the passage of the low center was immediately preceded by a northerly wind, and in every case (but two) the passage of the low center was immediately followed by a wind from the SE. These two cases occurred in storm No. 12, whose progress was almost exactly toward the north, and the low center was followed by a wind from the south at Navassa and by a wind from the SW. at Santiago. In all cases, the SE. wind showed indications of being a steady wind, resulting from causes of a more permanent character than the storms here considered, for it generally continued for several days; and at certain stations, where the fluctuation of the barometer was small, the wind blew from the SE. during the entire six days here represented.

34. I have endeavored to determine the average direction of the wind for the three months, August, September, and October (which months include nearly all the tropical cyclones before enumerated), for that part of the Atlantic Ocean in which these cyclones have most frequently occurred. Table V is derived from Maury's Pilot Charts of the North Atlantic, and shows the number of times the wind was observed to blow from the different points of the compass, in each of the five-degree squares from latitude 15° to 25° N. and longitude 50 to 75° W. from Greenwich.

The 11th, 22d, and 33d horizontal lines of the table show the sum of the observations of each month for each of the sixteen wind directions, and the last column of the table shows the average direction of the wind computed from these numbers.

35. Table VI gives the results of all the observations collected by the U. S. Hydrographic Office, including Maury's charts and the charts of the British Meteorological Office. The numbers represent the percentage of winds from sixteen points of the compass for each of the five-degree squares. When the sum of the numbers in any horizontal line is less than 100 the difference represents the cases of calms and variables.

This table makes the average direction of the wind somewhat more northerly than Table V.

According to Table V, the average direction of the wind for the three months here considered is two degrees north of east. According to Table VI it is 1½ degrees north of east. The average

TABLE V.—*Observations of the wind from Maury's Pilot Charts of the Atlantic Ocean.*

Latitude.	Longitude.	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.	Course.
								AUGUST.										
20° to 25°	50° to 55°	4	0	41	13	18	2	11	1	1	0	2	2	0	0	0	0	
20–25	55–60	3	2	28	12	21	2	6	1	2	0	1	0	0	0	0	0	
20–25	60–65	0	1	9	19	12	15	7	1	1	4	0	0	0	0	0	0	
20–25	65–70	0	0	0	3	6	0	0	0	0	0	0	0	0	0	0	0	
20–25	70–75	0	0	3	0	3	1	0	0	0	0	0	0	0	0	0	0	
15–20	50–55	0	9	28	11	18	8	2	0	0	0	0	0	0	0	0	0	
15–20	55–60	0	0	17	4	3	7	1	0	0	0	0	0	0	0	0	0	
15–20	60–65	0	0	3	1	3	1	0	0	2	0	0	0	0	0	0	0	
15–20	65–70	0	0	1	1	4	0	0	0	1	0	0	0	0	0	0	0	
15–20	70–75	0	0	0	0	6	0	0	0	0	0	0	0	0	0	0	0	
		7	12	130	67	94	36	27	3	7	4	3	2	0	0	0	0	N. 73° E.
								SEPTEMBER.										
20° to 25°	50° to 55°	2	4	22	23	12	6	15	1	1	7	8	2	2	2	0	1	
20–25	55–60	2	17	27	11	23	12	34	1	3	2	4	0	1	0	0	0	
20–25	60–65	1	1	9	0	4	2	9	0	2	1	3	0	0	0	0	0	
20–25	65–70	0	0	0	0	1	0	1	0	4	0	0	0	0	0	0	0	
20–25	70–75	0	0	0	0	1	0	1	1	0	0	0	0	0	0	0	0	
15–20	50–55	0	0	12	19	2	0	2	0	7	0	0	0	0	0	0	0	
15–20	55–60	0	0	3	1	9	4	3	0	0	0	0	0	0	0	0	0	
15–20	60–65	0	0	3	0	0	0	0	0	0	0	0	0	0	0	0	0	
15–20	65–70	0	0	0	0	4	6	2	0	1	0	0	0	0	0	0	0	
15–20	70–75	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	
		5	22	76	54	56	30	67	3	18	10	15	2	3	2	0	1	East.
								OCTOBER.										
20° to 25°	50° to 55°	1	1	14	9	22	16	18	3	6	6	3	0	2	0	1	0	
20–25	55–60	6	4	10	4	12	2	27	6	7	1	3	2	1	0	3	0	
20–25	60–65	0	0	12	5	6	7	9	1	0	2	9	2	0	1	1	0	
20–25	65–70	0	0	1	1	5	7	2	4	0	0	4	0	0	0	0	0	
20–25	70–75	0	0	1	0	7	8	2	0	0	0	0	3	0	0	0	0	
15–20	50–55	1	3	18	10	22	12	7	0	2	0	3	0	1	0	0	0	
15–20	55–60	1	1	8	17	12	6	13	3	1	0	0	0	0	0	0	0	
15–20	60–65	0	0	1	1	1	0	4	1	0	0	0	0	0	0	0	0	
15–20	65–70	0	0	3	0	6	0	12	0	0	0	0	0	0	0	0	0	
15–20	70–75	0	0	7	1	4	0	2	0	0	0	0	0	0	0	0	0	
		9	9	75	48	97	58	96	18	16	9	22	7	4	1	5	0	S. 79° E.

course of the storms mentioned in Table III, while moving westward, was 26° north of west; that is, they came from a point 26° south of east, which differs 28° from the average course of the wind by Table V, and differs 30° by Table VI. It is clear, then, that the West India cyclones do not follow the average direction of the wind for the region in which they occur. Tables V and VI, however, show that winds from the SE. and ESE. are very common, and the observations quoted in article 33 show that SE. winds very generally succeed a West India cyclone. These facts seem to indicate that the direction of a cyclone's progress is not determined by the direction of the prevalent wind for that season of the year so much as by the direction of the principal wind which prevails at the time of the cyclone.

36. I next undertook an investigation of the cyclones originating in the region south of the continent of Asia. Table VII contains various particulars respecting those cyclones of this region whose paths have been best determined. It includes all those which were most carefully investi-

gated by Henry Piddington, together with those which have been since investigated by Blanford, Elliott, and others. Column 1 gives the number of reference; column 2 shows the date of commencement, so far as indicated by the published observations; column 3 shows the latitude of the storm's center when it first became violent; column 4 shows the average course of the storm while advancing westward; column 5 shows the velocity of progress in English statute miles per hour

TABLE VI.—*Observations of the wind from the charts of the U. S. Hydrographic Office.*

AUGUST.

Latitude.	Longitude.	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.	Course.
20° to 25°	50° to 55°	1	1	45	24	14	6	6	....	....	....	1	....	....	....	1	....	
20–25	55–60	1	2	29	21	27	12	4	2	1	....	....	....	1	....	....	....	
20–25	60–65	0	3	12	23	22	13	12	4	2	3	1	2	....	....	1	....	
20–25	65–70	2	4	16	26	28	6	6	6	1	1	....	....	....	....	....	....	
20–25	70–75	2	0	17	17	29	14	6	1	5	....	1	....	....	....	....	....	
15–20	50–55	1	8	44	23	18	4	2	....	....	....	....	....	....	....	....	....	
15–20	55–60	0	0	33	25	26	13	2	1	....	....	....	....	....	....	....	....	
15–20	60–65	1	2	9	28	41	15	4	....	....	....	....	....	....	....	....	....	
15–20	65–70	1	2	9	31	25	12	11	2	2	1	2	....	....	....	....	1	
15–20	70–75	6	1	17	14	31	12	11	2	1	....	....	....	....	....	....	....	
		15	23	231	232	261	107	64	18	12	5	5	2	1	0	2	1	N. 78° E.

SEPTEMBER.

Latitude.	Longitude.	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.	Course.
20° to 25°	50° to 55°	2	6	19	18	10	6	12	5	1	2	2	1	2	1	....	....	
20–25	55–60	1	6	23	14	15	8	18	1	2	1	3	0	1	....	....	....	
20–25	60–65	2	3	16	12	18	12	15	6	5	1	2	1	1	....	1	1	
20–25	65–70	0	6	22	17	16	18	9	4	3	6	0	1	0	1	0	1	
20–25	70–75	1	1	11	11	21	6	15	8	9	5	5	4	....	....	....	....	
15–20	50–55	0	1	29	29	20	8	6	0	5	0	1	1	....	....	....	....	
15–20	55–60	1	6	21	18	36	11	5	0	....	....	....	....	2	....	....	....	
15–20	60–65	2	6	14	4	21	12	19	1	1	5	5	4	5	....	1	....	
15–20	65–70	0	2	15	11	20	25	14	4	3	1	....	....	....	....	1	1	
15–20	70–75	5	2	21	12	26	15	6	4	2	0	2	....	....	....	1	....	
		14	39	191	146	203	121	119	33	31	21	20	12	11	2	4	3	N. 88½° E.

OCTOBER.

Latitude.	Longitude.	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.	Course.
20° to 25°	50° to 55°	5	2	18	15	19	12	12	2	5	3	1	1	1	1	1	....	
20–25	55–60	4	1	17	11	15	7	19	4	4	1	5	1	2	0	2	1	
20–25	60–65	1	3	11	11	15	14	18	2	2	1	5	2	0	0	2	....	
20–25	65–70	1	5	11	4	18	19	11	10	2	3	11	1	....	....	....	1	
20–25	70–75	2	1	8	12	37	11	6	4	3	2	2	2	....	....	....	1	
15–20	50–55	2	4	28	20	14	8	9	0	2	0	5	0	1	1	1	1	
15–20	55–60	4	5	13	14	19	11	17	1	1	0	1	0	2	1	0	4	
15–20	60–65	3	2	11	19	18	9	20	4	10	1	1	....	....	....	....	1	
15–20	65–70	6	1	19	4	29	11	21	2	....	....	....	....	....	....	....	2	
15–20	70–75	6	4	21	18	18	1	5	5	4	....	....	1	1	....	....	....	
		34	28	157	128	202	103	138	34	33	11	31	8	7	3	6	11	East.

while moving westward; column 6 shows the latitude at which the course of the storm became due north; column 7 shows the velocity while moving north; column 8 shows the average course of the storm after turning eastward; column 9 shows the hourly velocity of progress while moving eastward; column 10 shows whether rain was mentioned as accompanying the storm, and whether the rain fall was violent or not; column 11 indicates the name of the person by whom the phenomena of the storm were investigated, and column 12 shows where the record of the investigation may be found.

37. It will be seen that 52 per cent. of these cases occurred in the months of September, October, and November, and 43 per cent. occurred in the months of April, May, and June, leaving

only 5 per cent. of the cases for the six remaining months of the year. Of the West India cyclones previously reported 88 per cent. occurred in the months of August, September, and October, leaving only 12 per cent. for the remaining nine months of the year: that is, the Asiatic cyclones occur in the spring almost as frequently as in the autumn; but the West India cyclones are almost exclusively confined to the period near the autumnal equinox.

The lowest latitude of any storm path here recorded is 6°.1, and there are fourteen cases below latitude 12°. The lowest latitude of any of the West India cyclones is 10°.3, and there are only three cases as low as latitude 12°.

TABLE VII.—*Course of cyclones originating near the China Sea, Bay of Bengal, &c.*

No.	Date of commencement.	Latitude of origin.	Course while moving westward.	Velocity in miles per hour.	Latitude when moving north.	Velocity moving north.	Course while moving eastward.	Velocity moving eastward.	Rainfall.	Investigator.	Where recorded.
1	1803, Sept. 21	16.0°	W. 15 N.	9.1					Heavy	Piddington	Jo. Asia. Soc., v. 11.
2	1810, Sept. 28	18.1	W. 12 S.	7.3					Heavy	Piddington	Jo. Asia. Soc., v. 11.
3	1835, Aug. 5	20.5	W. 18 N.	17.0					Heavy	Redfield	Jo. Science, v. 35.
4	1838, Apr. 8	22.6					S. 37 E.	5.0	Hail	Floyd	Jo. Asia. Soc., v. 7.
5	1839, June 3	20.0	W. 13 S.	3.9					Violent	Piddington	Jo. Asia. Soc., v. 8.
6	Sept. 20	22.0	W. 52 N.	9.5					Violent	Piddington	Jo. Asia. Soc., v. 9.
7	Nov. 12	13.3	W. 23 N.	6.2					Heavy	Piddington	Jo. Asia. Soc., v. 9.
8	1840, Apr. 27	11.6	W. 54 N.	9.8					Violent	Piddington	Jo. Asia. Soc., v. 9.
9	Sept. 22	15.6	W. 83 N.	10.0					Violent	Piddington	Jo. Asia. Soc., v. 10.
10	1841, May 15	10.0	W. 25 N	14.7					Heavy	Piddington	Jo. Asia. Soc., v. 11.
11	1842, June 2	20.5	W. 69 N.	4.8					Violent	Piddington	Jo. Asia. Soc., v. 11.
12	Oct. 1	17.7	W. 31 N.	7.5	24.3	4.6			Violent	Piddington	Jo. Asia. Soc., v. 12.
13	Oct. 22	12.0	W.	12.1					Violent	Piddington	Jo. Asia. Soc., v. 12.
14	1843, May 20	8.8	W. 38 N.	12.1					Heavy	Piddington	Jo. Asia. Soc., v. 13.
15	Nov. 28	6.1	W. 40 N.	4.6					Rain	Piddington	Jo. Asia. Soc., v. 14.
16	1844, Nov. 9	11.1	W. 16 N.	3.4					Heavy	Piddington	Jo. Asia. Soc., v. 14.
17	1845, Oct. 7	17.1	W. 19 N.	13.5					Rain	Piddington	Jo. Asia. Soc., v. 18.
18	Nov. 29	6.7	W. 12 N.	6.0					Violent	Piddington	Jo. Asia. Soc., v. 14.
19	1847, Apr. 16	7.9	W. 86 N.	9.4					Violent	Piddington	Jo. Asia. Soc., v, 17.
20	Nov. 18	17.0	W. 49 N.	6.2	18.5	5.8	N. 53 E.	5.0	Rain	Piddington	Jo. Asia. Soc., v. 18.
21	1848, Oct. 12	17.8	W. 50 N.	4.8					Rain	Piddington	Jo. Asia. Soc., v. 18.
22	1850, Apr. 23	8.7	W. 50 N.	8.1	18.0	9.1			Violent	Piddington	Jo. Asia. Soc., v. 20.
23	Nov. 17	12.2	W. 70 N.	6.0					Heavy	Piddington	Jo. Asia. Soc., v. 23.
24	1851, May 2	10.6	W. 54 N.	3.6					Violent	Piddington	Jo. Asia. Soc., v. 21.
25	Oct. 21	17.6			17.6	10.9	N. 42 E.	5.7	Rain	Piddington	Jo. Asia. Soc., v. 23.
26	1852, May 12	15.7			20.0	10.1			Rain	Piddington	Jo. Asia. Soc., v. 24.
27	1854, Apr. 22	13.2					N. 39 E.	9.8	Heavy	Piddington	Jo. Asia. Soc., v. 27.
28	1856, Dec. 7	10.0	W. 24 N.	11.7					Violent	Maury	Sailing Directions, v. 1.
29	1858, Apr. 9	14.2					N. 39 E.	12.4	Violent	Liebig	Jo. Asia. Soc., v. 27.
30	1864, Oct. 3	16.0	W. 56 N.	9.2	21.3	12.0	N. 29 E.	15.0	Violent	Gastrell	Special Report.
31	1869, May 13	16.0	W. 41 N.	7.6	20.5	14.0	N. 25 E.	17.0	Rain	Blanford	Special Report.
32	June 5	16.4	W. 83 N.	4.0	24.0	9.5	N. 39 E	11.0	Violent	Blanford	Special Report.
33	Oct. 7	20.5	W. 34 N.	12.5					Violent	Blanford	Special Report.
34	1870, Nov. 4	16.5	W. 11 N.	12.0					Violent	Blanford	Special Report.
35	1872, Apr. 28	7.5	W. 50 N.	5.0					Heavy	Blanford	Special Report.
36	June 28	20.5	W. 21 N.	7.1					Violent	Blanford	Special Report.
37	Sept. 19	21.0			23.0	12.7	N. 13 E.	13.7	Violent	Blanford	Special Report.
38	1874, May 3	9.0	W. 39 N.	7.0					Violent	Blanford	Special Report.
39	Oct. 13	16.6	W. 60 N.	6.9	22.1	8.3	N. 40 E.	9.4	Violent	Willson	Special Report.
40	1876, Oct. 6	11.4	W. 25 N.	7.5	17.6	6.0	N. 16 E.	8.0	Heavy	Elliott	Special Report.
41	Oct. 27	11.0			14.0	10.0	N. 17 E.	20.0	Violent	Elliott	Special Report.
42	1877, May 14	9.3	W. 63 N.	5.0	15.0	7.9	N. 45 E.	10.4	Violent	Elliott	Special Report.

The courses of these storms while moving westward range from 13° south of west to 86° north of west, the average direction being 38° north of west. In two cases the course was reported to be south of west, and in one case it was exactly west, which result accords very closely with that before found for West India cyclones. The average velocity of progress of these storms while advancing westward was 8.1 English statute miles per hour, which is less than half the average velocity of West India cyclones.

The average latitude of the storm centers, when the course became due north, was 19°.8, and the latitudes range from 14° to 24°.3, which is ten degrees more southerly than the latitude before

found for the West India cyclones. The average velocity of progress of these storms when advancing northward was 9.3 miles per hour.

The average course of these storms, after turning eastward, was 35° east of north, and their velocity of progress was 9.8 miles, which is scarcely half of the velocity found for West India cyclones.

Column 10 shows that rain accompanied every one of these storms, and generally the rain-fall was excessively great. These observations were generally made from vessels on the ocean, and the amount of the rainfall could not be measured, but the rain was generally characterized by the strongest terms which the English language furnishes, such as, very heavy rain—constant heavy rain—ceaseless rain—excessively heavy rain—incessant heavy rain—sheets of rain—deluge of rain—rain poured down in torrents—dense, thick, impenetrable rain—rain with a vengeance—rain and very large hail—rain and sleet—hard sleet—torrents of rain and sleet, &c.

38. When a storm center passed overland where a rain-gauge was observed the measurements showed that the preceding terms were no exaggeration. The following table shows the amount of rainfall in twenty-four hours at certain stations within the limits of the cyclones named in Table VII:

TABLE VIII.—*Rainfall in tropical cyclones.*

Date.	Place.	Latitude.	Longitude.	Rain, inches.	Authority.
1839, June 4	Dacca	23. 7°	90. 5°	6. 00	Piddington, 1st Memoir, p. 37.
1842, June 3	Calcutta	22. 5	88. 3	5. 17	7th Memoir, p. 35.
June 3	Kissenuggur	23. 4	88. 4	9. 00	7th Memoir, p. 42.
Oct. 3	Pooree	19. 8	85. 9	5. 10	9th Memoir, p. 27.
1843, May 23	Cannanore	11. 9	93. 2	5. 95	10th Memoir, p. 32.
May 23	Madras	13. 1	80. 3	10. 50	10th Memoir, p. 20.
May 23	Hyderabad	25. 3	68. 4	9. 00	10th Memoir, p. 29.
1851, May 5	Madras	13. 1	80. 3	11. 44	21st Memoir, p. 17.
1864, Oct. 6	Contai	21. 8	87. 8	10. 00	Rep. of Gastrell & Blanford, p. 82.
Oct. 6	Bograh	24. 8	89. 4	7. 10	p. 82.
Oct. 6	Goalparah	26. 2	90. 7	60. 00	p. 82.
Oct. 6	Moisgunj	23. 4	88. 5	7. 50	p. 82.
1874, May 4	Madras	13. 1	80. 3	7. 10	Wilson's Report, p. 127.
Oct. 15	False Point	20. 3	86. 8	6. 30	p. 9.
Oct. 15	Jellasore	21. 5	86. 9	5. 82	p. 9.
Oct. 15	Midnapore	22. 4	87. 2	10. 27	p. 8.
Oct. 16	Burdwan	23. 2	87. 9	7. 43	p. 8.
Oct. 16	Lalgolla	24. 5	88. 3	16. 30	p. 8.
Oct. 16	Jungipore	24. 5	87. 8	8. 00	p. 8.
Oct. 16	Bood Bood	23. 0	88. 0	8. 40	p. 8.
Oct. 17	Rungpore	25. 9	89. 3	6. 97	p. 9.
1876, Oct. 7	Vizagapatam	17. 7	83. 4	5. 60	Elliott's Report, p. 48.
Oct. 8	Vizagapatam	17. 7	83. 4	12. 60	p. 48.
Nov. 1	Noakholly	22. 8	91. 0	5. 12	p. 153.
Nov. 1	Putuakhally	22. 3	90. 4	5. 85	p. 153.
1877, May 18	Madras	13. 1	80. 3	13. 01	p. 42.
May 20	Gya	24. 6	85. 1	5. 06	p. 75.
May 20	Nowada	23. 9	88. 4	8. 00	p. 75.
May 20	Aurungabad	19. 9	75. 3	8. 68	p. 75.
May 20	Rajmahal	25. 0	87. 7	5. 20	p. 75.
May 20	Raigunge	25. 0	88. 0	5. 71	p. 75.
May 20	Jawai	25. 0	91. 0	9. 70	p. 75.
May 21	Barrh	25. 5	95. 7	6. 43	p. 77.
May 21	Chanchal	25. 0	88. 2	6. 14	p. 77.
May 21	Rungpore	25. 9	89. 3	11. 16	p. 77.
May 21	Kurigram	25. 0	89. 0	5. 70	p. 77.
May 21	Bogdogra	25. 0	89. 0	12. 19	p. 77.
May 21	Julpigoree	26. 5	88. 7	5. 53	p. 77.
May 21	Boda	26. 0	89. 0	8. 52	p. 77.
May 21	Cooch Behar	26. 3	89. 5	9. 77	p. 77.
May 21	Dhubri	26. 0	90. 0	5. 60	p. 77.
May 21	Jawai	25. 0	91. 0	14. 20	p. 77.

From this table we see that these cyclones were accompanied by an amount of rain such as seldom occurs even within the tropics, and we seem authorized to conclude that excessive rain

invariably accompanies the most violent cyclones. This conclusion accords with that deduced from the investigation of the West India cyclones.

39. I next examined all the maps of the international observations for additional materials showing the course of storms in Southern Asia and the adjacent oceans. The following are the most important cases which I have found of storms advancing in a westerly direction:

TABLE IX.—*Asiatic storms advancing westerly.*

No.	Date.			Latitude. Beg. End.	Longitude. Beg. End.	Course.	Velocity, miles.	Subsequent course.
1	1878,	Sept.	15–19	15–30°	134–125°	NNW.	10.8	Moved NE.
2		Oct.	7– 9	19–19	122–110	W.	14.3	Unknown.
3		Nov.	17–21	12–15	95– 82	W.	7.2	Unknown.
4		Nov.	29–38	12–20	97– 81	W. & NW.	5.8	Unknown.
5	1879,	Apr.	17–23	13–20	134–126	W. & NW.	5.4	Moved NE.
6		May	30–32	20–22	88– 90	W. & N.	10.8	Moved NE.
7	1880,	July	13–19	6–21	121–106	NW.	8.4	Unknown.
8		July	25–33	15–16	132–107	W.	7.8	Unknown.
9		July	30–39	18–30	143–128	NW.	5.6	Moved northward.
10		Aug.	25–32	13–19	125–107	WNW.	7.2	Unknown.
11		Sept.	14–19	16–17	127–106	W.	9.6	Unknown.
12		Sept.	18–25	11–19	127-106	NW. & W.	7.0	Unknown.
13		Sept.	26–28	19–22	129–113	W.	17.2	Unknown.
14		Oct.	10–17	17–18	127–108	W.	6.3	Unknown.
15		Oct.	24–28	9–22	120–122	N.	7.7	Unknown.
16		Nov.	9–14	10– 7	118–103	W.	7.2	Unknown.
17	1881,	May	22–31	9–17	128–111	WNW.	7.5	Moved eastward.
18		June	26–35	10–29	126–121	NW.; N. & NE.	8.3	Moved NE.
19		July	6–11	9–20	123–106	WNW.	10.0	Unknown.
20		July	10–17	8–30	128–122	NNW.	10.0	Moved NE.
21		Aug.	9–16	24–24	136–109	NW. & W.	10.4	Unknown.
22		Aug.	18–24	12–22	127–108	WNW.	8.6	Unknown.
23		Aug.	23–30	17–30	130–124	NW. & N.	8.3	Unknown.
24		Sept.	6–10	14–28	126–112	NW.	8.6	Unknown.
25		Sept.	29–40	14–22	126–107	W. & NW.	8.6	Moved NE.
26		Oct.	12–18	16–30	126–119	NW.	11.5	Moved NE.
27		Oct.	18–30	12–30	124–106	W. & NW.	6.6	Moved NE.
28		Nov.	5–12	10– 9	125–108	W	9.6	Unknown.
29		Nov.	26–29	9–14	127–108	WNW.	14.3	Unknown.

On Plate XII, Fig. 2, these tracks are delineated, and are designated by the same numbers as in the table. The average direction of progress of these storms in the early part of their course was $27\frac{1}{2}°$ north of west, and their average rate of progress was 9 miles per hour. This velocity corresponds closely with that deduced from Table VII, but the direction corresponds more nearly with that found for the West India cyclones.

40. I next endeavored to compare this average direction of storm paths with the average direction of the wind in the same region. In the Bay of Bengal and in the China Sea the average direction of the wind is from the northeast in winter and from the southwest in summer. In order to make a satisfactory comparison between the average direction of the wind and that of the progress of storms we must make a separate comparison for the different seasons of the year; and since the winds of the China Sea differ somewhat from those of the Bay of Bengal, I will restrict the comparison to the China Sea, and omit the storms numbered 3, 4, and 6, which occurred in the Bay of Bengal.

Of the remaining twenty-six storms we perceive that none occurred in the months of December, January, February, and March, and only one occurred in each of the months of April, May, and June. Five occurred in July, four in August, six in September, five in October, and three in November. We will therefore restrict the comparison to the five months from July to November. The following table is derived from Maury's Pilot Chart of the China Sea, and shows for these months the number of times the wind was observed to blow from the different points of the compass in each of the five degree squares from latitude 10° to latitude 20° N., and longitude 110° to longitude 125° E. from Greenwich.

TABLE X.—*Observations of the wind from Maury's Pilot Charts of the Pacific Ocean.*

Latitude.	Longitude.	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.	Course.
									JULY.									
15° to 20°	110° to 115°	2	0	5	6	7	2	18	29	54	38	26	5	12	4	1	0	
15–20	115–120	2	0	7	0	0	0	2	2	2	33	24	13	3	0	3	1	
15–20	120–125	0	0	6	3	2	2	4	1	3	1	3	0	1	2	2	0	
10–15	110–115	1	0	2	2	6	4	26	14	27	57	61	18	9	6	3	1	
10–15	115–120	3	0	3	1	5	4	16	7	13	13	43	7	15	3	7	3	
10–15	120–125	0	1	0	0	0	3	7	1	16	7	17	10	5	1	14	1	
		8	1	23	12	20	15	73	54	115	149	174	53	45	16	30	6	S. 22° W.
									AUGUST.									
15° to 20°	110° to 115°	8	6	1	7	2	1	17	8	28	38	46	21	9	1	4	2	
15–20	115–120	3	0	4	0	0	3	1	1	12	13	38	6	0	1	7	1	
15–20	120–125	0	0	0	6	0	0	0	0	0	0	0	3	0	0	0	0	
10–15	110–115	8	0	4	1	1	0	4	3	13	23	135	19	17	6	5	0	
10–15	115–120	0	0	1	1	3	3	2	5	4	25	39	11	11	1	2	0	
10–15	120–125	5	0	1	1	1	1	3	0	16	0	14	0	0	0	5	1	
		24	6	11	16	7	8	27	17	73	99	272	60	37	9	23	4	S. 39° W.
									SEPTEMBER.									
15° to 20°	110° to 115°	22	12	31	15	34	9	15	10	18	10	9	2	7	7	3	3	
15–20	115–120	10	2	20	1	13	12	25	6	29	1	29	6	12	7	12	20	
15–20	120–125	0	0	0	3	0	0	0	0	0	0	0	0	3	0	0	0	
10–15	110–115	19	5	9	0	1	0	6	8	40	54	139	39	39	7	23	4	
10–15	115–120	6	1	5	4	1	0	8	4	21	13	49	8	15	3	10	3	
10–15	120–125	1	1	6	3	0	0	5	2	7	6	4	9	3	1	1	5	
		58	21	71	29	49	21	59	30	115	81	230	64	79	25	49	35	S. 39° W.
									OCTOBER.									
15° to 20°	110° to 115°	10	18	67	15	33	4	9	4	3	1	2	0	0	0	1	1	
15–20	115–120	36	16	111	22	35	7	15	7	21	5	7	1	12	6	5	3	
15–20	120–125	0	0	3	3	0	0	0	0	0	0	0	0	0	2	0	0	
10–15	110–115	43	19	74	11	21	11	15	22	23	10	22	6	8	8	24	15	
10–15	115–120	32	13	34	10	38	14	6	13	14	1	14	7	5	3	13	10	
10–15	120–125	6	0	2	3	4	1	5	2	2	0	1	0	0	0	4	0	
		127	66	291	64	131	37	50	48	63	17	46	14	25	19	47	29	N. 53° E.
									NOVEMBER.									
15° to 20°	110° to 115°	5	21	41	14	6	2	0	0	0	0	0	0	0	0	0	2	
15–20	115–120	43	24	107	13	14	5	5	5	7	0	0	0	3	5	11	1	
15–20	120–125	3	3	5	0	0	0	1	0	0	0	0	0	0	0	0	0	
10–15	110–115	30	19	103	34	19	5	7	3	1	0	4	0	1	0	8	7	
10–15	115–120	31	13	82	26	25	2	3	3	6	0	11	1	3	3	20	5	
10–15	120–125	2	0	3	0	0	3	2	0	0	1	1	0	3	0	0	0	
		114	80	344	87	64	17	18	11	14	1	16	1	10	8	39	15	N. 41° E.

The average course of the winds deduced from these numbers is, for July, S. 22° W.; for August, S. 39° W.; for September, S. 39° W.; for October, N. 53° E.; and for November, N. 41° E. For the first three months the average direction of the winds is S. 33° W., and for the last two months it is N. 47° E. For the first three months the average direction of progress of storms (as deduced from Table VII, combined with Table IX) is 35° north of west, and for the last two months it is 25½° north of west. That is, a change of 166° in the average direction of the wind

is accompanied by a change of only 9½° in the average direction of the progress of storms. This fact clearly indicates that the direction in which storms advance is mainly determined by some other cause than the mean direction of the wind.

41. I next endeavored to ascertain what was the prevalent direction of the wind which preceded each of the storms referred to in Table IX, and also the prevalent wind which succeeded the low center. The observations published in the United States International Bulletin include only two stations within the tropics in the neighborhood of the China Sea, viz, Manila and Tuguegarao, both of them on the island of Luzon, one of the Philippine Islands (see Plate V), and the observations at the latter station commence with the year 1881. The following table shows the observations at these two stations for all those storms of 1881 which passed near enough to either of these stations to cause a decided fall of the barometer. The observations include (1) The barometer in English inches reduced to sea-level; (2) the thermometer (Fahrenheit); (3) the relative humidity; (4) the wind's direction; (5) the wind's velocity in miles per hour; and (6) the rainfall, in English inches, during the preceding twenty-four hours:

TABLE XI.—*Observations near the time of cyclones.*

1881.	MANILLA. Barom.	Therm.	Hum.	Wind. Direction.	Wind. Velocity.	Rain.	TUGUEGARAO. Barom.	Therm.	Hum.	Wind. Direction.	Wind. Velocity.	Rain.
June 27	29.85	82.6°	87	NNE.	1.1	0.08	29.92	80.1°	88	ESE.	0.4	0.09
28	.51	78.7	100	W.	87.2	4.71	.84	79.2	92	NE.	0.0	0.25
29	.83	77.4	82	ESE.	8.8	5.48	.75	79.7	84	SW.	4.3	0.06
30	.82	78.7	77	SE.	2.2	2.09	.76	75.9	91	S.	11.5	0.39
31	.83	82.1	81	E.	2.2	0.16	.82	85.0	78	S.	1.3	0.04
July 6	.80	83.0	72	SW.	11.4	0.04	.81	83.2	71	NNW.	4.2	0.00
7	.78	80.5	69	SW.	12.1	3.39	.81	85.5	73	ENE.	2.0	0.04
8	.73	81.0	75	SSW.	11.4	1.00	.76	78.0	92	NW.	0.0	0.93
9	.87	79.6	65	ESE.	2.2	0.87	.87	77.8	94	S.	9.7	0.08
10	.91	80.6	66	ENE.	5.5	0.15	.97	81.5	87	S.	1.3	0.07
July 11	.88	84.1	72	SW.	6.6	0.00	.90	80.7	78	N.	3.7	0.01
12	.76	83.7	79	W	10.3	0.04	.80	80.0	85	NNW.	4.4	0.00
13	.63	84.1	77	WSW.	22.0	0.22	.54	78.8	93	NNW.	11.0	2.40
14	.71	82.4	85	SW.	17.9	0.20	.65	75.5	94	SSE.	8.4	1.67
15	.81	82.4	84	SSW.	5.9	0.31	.79	79.4	90	SW.	1.3	0.16
Aug. 18	.85	80.5	92	NW.	6.6	2.41	.87	80.2	78	NNW.	4.7	0.04
19	.50	78.8	98	WSW.	88.4	2.17	.51	77.0	92	N.	14.3	0.65
20	.76	79.2	83	SE.	9.9	4.68	.75	77.3	83	SSE.	8.3	0.70
21	.84	83.0	82	SSW.	8.4	0.00	.80	83.6	84	S.	0.0	0.00
Aug. 24	.81	83.9	78	WSW.	16.5	0.00	.72	82.8	73	N.	0.9	0.00
25	.84	83.3	81	WSW.	17.6	0.00	.64	81.7	81	SE.	3.2	0.00
26	.87	81.7	90	SW.	8.4	0.98	.76	83.8	75	SW.	4.7	0.00
27	.94	83.5	82	SW.	15.7	0.00	.87	84.4	74	N.	4.9	0.04
Sept. 5	.80	83.9	82	SW.	12.8	0.00	.81	80.0	83	NNW.	4.5	0.00
6	.68	84.4	80	W.	20.9	0.00	.56	81.9	79	NNW.	2.7	0.00
7	.77	82.3	86	SSW.	14.7	0.61	.69	81.0	88	NW.	0.7	0.05
8	.82	79.6	92	SSW.	4.4	0.42	.82	79.0	87	SE.	5.4	0.01
9	.81	79.4	91	E.	6.6	0.11	.82	82.1	84	SW.	0.2	0.04
Oct. 11	.82	84.4	77	SSW.	8.8	0.00	.89	78.2	87	NNW.	5.1	0.00
12	.65	82.6	89	SW.	46.2	0.77	.30	75.2	98	SE.	40.7	0.76
13	.86	81.9	85	SW.	4.4	0.82	.84	79.2	85	S.	1.7	0.03
Oct. 17	.82	82.4	79	NNE.	7.7	0.00	.85	77.8	91	NW.	3.1	0.14
18	.72	75.1	95	NNE.	0.7	0.22	.72	84.2	98	NNW.	9.6	1.28
19	.58	80.2	81	W.	34.8	0.55	.22	74.9	97	NNW.	50.0	2.04
20	.68	79.2	83	SSW.	3 .7	0.45	.55	80.0	78	S.	16.1	0.20
21	.82	78.4	95	SSE.	3.3	0.65	.82	78.9	86	S.	7.6	0.00

42. We see from this table that during the months from June to September, at both of these stations, in two thirds of the cases, the cyclone was followed by a wind from some point between ESE. and south, and this wind generally lasted more than twenty four hours. In the remaining cases, the wind which followed the cyclone was from the SSW. or SW., and in these cases the center of the cyclone passed on the east side of the given station. In the month of October (when the prevalent wind is from the northeast) each cyclone was followed by a wind from some point between south and southwest. and this southerly wind lasted more than twenty-four hours. It appears, then, that in the China Sea cyclones are generally succeeded by a southerly wind of considerable duration, even in those months in which the average wind is northerly.

From Mr. Elliott's investigations of cyclones in the Bay of Bengal, particularly the cyclone of October 8–19, 1882, it appears that these southerly winds, which prevail on the south side of a cyclone, extend down to the equator as strong winds, accompanied by severe squalls and rain. During a tropical cyclone these southerly winds appear to push forward with greater persistence than the northerly winds, and this seems to be at least a part of the reason for the northerly progress of the cyclone.

These results accord substantially with those found in article 33 for West India cyclones, and show that the average direction of the progress of cyclones does not coincide with the average direction of the wind for the same season of the year, but corresponds more nearly to that of the principal wind prevailing at the time of the cyclone. It is not, however, claimed that there is an exact agreement between these two directions.

43. An examination of Plate XI shows that in the middle latitudes of the northern hemisphere there is a remarkable correspondence between the average direction of the progress of storm centers and the average direction of the wind as shown on Coffin's wind charts. I have endeavored to ascertain whether this correspondence is exact, or whether there is a constant difference between these two directions. I first made a comparison of these two directions for the Atlantic Ocean.

44. In order to determine the average direction of progress of storm centers across the Atlantic Ocean I measured with a protractor the bearing of the storm tracks delineated on the United States International Charts. These bearings were measured for six points, viz, at the intersection of the storm tracks with the meridians of 10°, 20°, 30°, 40°, 50°, and 60° west of Greenwich, and the measurements included the observations of four years, viz, 1878–1881. Table XII shows the average results of these measurements for each month of the year and for each of the six points above mentioned. The latitudes named at the top of the table are the average latitudes corresponding to the given directions:

TABLE XII.—*Direction of storm tracks.*

	Longitude 60°. Latitude 46.9°.	Longitude 50°. Latitude 48.9°.	Longitude 40°. Latitude 51.3°.	Longitude 30°. Latitude 53.9°.	Longitude 20°. Latitude 54.9°.	Longitude 10°. Latitude 55.5°.
January......	N. 66° E.	N. 61° E.	N. 61° E.	N. 74° E.	N. 86° E.	N. 96° E.
February.....	66	67	60	60	74	82
March	73	69	68	65	71	79
April	63	68	72	79	91	97
May	62	67	68	71	76	76
June.........	76	63	64	67	71	71
July	72	62	59	68	76	80
August	69	74	74	77	72	71
September....	67	72	78	75	75	73
October......	67	64	72	68	73	72
November....	70	67	62	69	68	69
December....	65	66	62	67	73	80
Year......	N. 68 E.	N. 67 E.	N. 67 E.	N. 70 E.	N. 75 E.	N. 79 E.

45. I next endeavored to determine the average direction of the wind for the entire year at several points on the Atlantic Ocean, as near as possible to the points corresponding to the preceding measurements. Table XIII is derived from Maury's Pilot Charts of the Atlantic Ocean, and shows the number of wind observations for sixteen points of the compass, for each of the

five-degree squares near the storm tracks delineated on the international charts. The direction of the winds, computed from these numbers, is shown in the last column of the table:

TABLE XIII.—*Wind observations from Maury's charts.*

Latitude.	Longitude.	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.	Course.
35 to 40°	65° to 60°	145	86	134	44	65	51	55	79	123	96	260	118	230	138	196	75	N. 88.3° W.
40–45	65–60	115	67	85	59	92	32	82	84	126	102	205	146	188	125	93	89	S. 73.0 W.
35–40	60–55	103	75	101	38	69	32	77	78	138	115	258	137	201	88	134	82	S. 71.1 W.
40–45	60–55	104	51	78	28	78	49	86	54	138	121	201	97	207	115	134	92	S. 74.3 W.
35–40	55–50	54	81	64	69	39	87	84	146	135	249	158	173	87	139	60	96	S. 37.0 W.
40–45	55–50	94	68	90	39	77	56	92	76	131	90	168	101	179	102	154	93	S. 79.4 W.
40–45	50–45	81	88	60	86	51	88	70	126	96	124	98	160	126	123	94	88	S. 63.7 W.
45–50	50–45	4	25	1	23	25	21	11	21	26	50	27	29	20	17	13	18	S. 20.0 W.
40–45	45–40	48	50	19	45	32	60	56	84	97	161	108	142	121	165	94	101	S. 67.8 W.
45–50	45–40	12	17	14	41	33	36	22	44	42	66	75	97	55	65	49	44	S. 58.8 W.
40–45	40–35	43	42	19	49	34	38	54	96	66	107	108	158	121	112	54	56	S. 58.7 W.
45–50	40–35	20	28	25	34	49	47	24	68	65	88	107	123	104	108	58	50	S. 61.0 W.
40–45	35–30	31	50	26	53	31	41	32	59	61	71	63	82	78	78	61	56	S. 71.3 W.
45–50	35–30	43	49	33	38	38	47	28	48	80	110	115	124	114	133	66	64	S. 71.8 W.
45–50	30–25	67	46	22	33	28	35	24	59	74	128	98	143	130	99	71	83	S. 75.8 W.
50–55	30–25	7	2	2	10	1	11	11	9	15	26	41	45	32	13	14	23	S. 63.2 W.
45–50	25–20	52	41	26	48	45	35	30	73	57	98	101	129	135	122	94	79	S. 81.1 W.
50–55	25–20	10	9	5	20	8	20	24	47	32	45	25	59	35	34	21	24	S. 42.3 W.
45–50	20–15	47	41	33	39	23	35	31	56	50	90	74	138	86	124	83	84	S. 86.8 W.
50–55	20–15	11	16	9	22	28	35	38	68	49	51	36	71	51	49	29	18	S. 30.4 W.
45–50	15–10	41	57	49	60	26	29	32	10	41	89	65	100	87	129	71	85	N. 78.8 W.
50–55	15–10	35	34	35	54	56	62	61	82	69	79	76	121	85	83	49	63	S. 43.5 W.
45–50	10– 5	15	13	19	12	22	22	19	11	20	22	23	14	20	19	23	18	S. 45.3 W.
50–55	10– 5	42	23	28	22	34	19	23	28	44	21	41	33	79	42	31	17	S. 82.2 W.

46. I have also determined the average direction of the wind for the same part of the Atlantic Ocean for the months of January, April, July, and October, according to the charts of the U. S. Hydrographic Office. Table XIV shows the percentage of the wind directions for five-degree squares, from sixteen points of the compass, as given by the charts. The last column shows the average directions computed from the sum of the numbers for the four given months:

TABLE XIV.—*Wind observations from the charts of the U. S. Hydrographic Office.*

Latitude.	Longitude.	Month.	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.	Course.
40° to 45°	65° to 60°	January	5	5	5	5	5	5	4	5	5	4	11	5	10	6	10	8	
		April	15	6	5	5	4	1	5	2	4	3	6	9	6	8	6	10	
		July	2	1	2	1	3	2	5	5	10	10	11	13	12	5	6	6	
		October	12	6	7	1	4	1	1	2	4	7	7	7	11	10	9	9	
		Sum	34	18	19	12	16	9	15	14	23	24	35	34	39	29	31	33	N. 76.7° W.
40–45	60–55	January	2	3	7	3	1	5	0	3	2	5	11	8	15	10	17	4	
		April	6	5	4	4	4	3	1	2	5	14	10	5	8	6	11	9	
		July	5	5	4	2	5	3	6	4	10	8	10	8	13	5	5	5	
		October	7	2	2	1	2	1	2	1	5	4	4	8	11	19	15	13	
		Sum	20	13	17	10	12	12	9	10	22	31	35	29	47	40	48	31	N. 81.9° W.
40–45	55–50	January	7	5	4	5	6	5	1	4	5	7	9	6	8	9	9	5	
		April	8	5	5	1	3	5	4	4	6	5	8	8	9	6	6	11	
		July	5	3	6	2	4	5	7	6	10	8	8	6	10	6	6	4	
		October	6	5	5	6	6	4	5	5	6	5	4	2	9	7	12	9	
		Sum	26	18	20	14	19	19	17	19	27	25	29	22	36	28	33	29	N. 82.9° W.
40–45	50–45	January	11	4	0	7	2	5	6	6	2	9	5	11	5	16	6	5	
		April	5	5	5	4	4	3	6	6	7	6	4	10	5	6	7	14	
		July	1	5	3	4	5	5	3	10	8	11	7	12	7	5	4	4	
		October	7	6	6	1	5	4	5	11	10	4	4	6	8	6	11	6	
		Sum	24	20	14	16	16	17	20	33	27	30	20	39	25	33	28	29	S. 73.3° W.

TABLE XIV.—*Wind observations from the charts of the U. S. Hydrographic Office*—Continued.

Latitude.	Longitude.	Month.	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.	Course.
45° to 50°	45° to 40°	January.	6	1	0	2	0	5	6	5	6	6	5	16	11	11	6	5	
		April....	4	0	0	4	5	5	8	10	2	6	8	6	5	5	21	8	
		July	1	1	2	5	2	3	2	7	4	16	12	18	6	2	9	8	
		October .	1	2	2	3	11	5	1	11	5	9	11	8	7	9	5	6	
		Sum..	12	4	4	14	18	18	17	33	17	37	36	47	29	27	41	27	S. 60.7° W.
45–50	40–35	January.	1	1	0	2	2	3	1	10	6	9	8	22	12	11	4	2	
		April....	6	2	5	5	3	2	2	4	2	4	16	5	9	5	14	10	
		July	1	1	1	0	2	4	2	12	9	8	13	15	11	10	6	2	
		October .	4	5	1	2	3	4	4	5	4	27	4	6	9	9	5	5	
		Sum..	12	9	7	9	10	13	9	31	21	48	41	48	41	35	29	19	S. 62.6° W.
45–50	35–30	January.	5	8	2	3	2	6	2	4	4	15	8	18	8	10	5	6	
		April....	4	4	4	5	2	11	5	3	5	7	10	7	8	11	4	5	
		July	3	2	4	3	1	2	4	9	8	4	9	20	10	8	6	5	
		October .	5	7	1	4	5	4	4	4	6	7	11	9	7	12	5	6	
		Sum..	17	21	11	15	10	23	15	20	23	33	38	54	33	41	20	22	S. 69.5° W.
45–50	30–25	January.	4	3	3	0	6	3	4	5	5	13	11	11	8	7	6	5	
		April....	11	8	4	4	4	6	2	4	3	10	5	11	9	5	5	4	
		July	5	3	3	6	2	2	4	12	4	11	10	8	5	5	6	9	
		October .	4	3	3	2	0	1	4	5	5	10	7	10	7	10	12	14	
		Sum..	24	17	13	12	12	12	14	26	17	44	33	40	29	27	29	32	S. 77.5° W.
45–50	25–20	January.	4	2	3	5	4	3	12	4	6	5	7	10	11	8	8	5	
		April....	6	5	4	4	10	4	3	5	5	4	3	10	6	10	9	8	
		July	1	1	0	3	2	3	2	6	5	6	11	17	12	14	7	4	
		October .	11	6	5	1	6	3	2	1	2	3	5	10	11	11	10	12	
		Sum..	22	14	12	13	22	13	19	16	18	18	26	47	40	43	34	29	N. 87.4 W.
50–55	20–15	January.	2	0	4	4	5	12	6	10	9	3	5	14	8	6	7	5	
		April....	6	1	4	3	6	3	7	7	4	11	10	10	7	8	6	4	
		July	1	1	1	4	1	2	3	4	5	14	8	18	20	5	6	2	
		October .	6	7	5	8	16	4	9	9	2	4	1	8	3	[illegible]	1	5	
		Sum..	15	12	14	19	28	21	25	30	20	32	24	50	38	22	20	16	S. 37.8° W.
50–55	15–10	January.	5	5	5	5	5	4	4	5	6	5	10	15	6	8	5	5	
		April....	5	4	4	2	5	0	7	5	11	11	7	5	8	11	5	5	
		July	6	1	5	5	2	5	2	3	4	12	6	15	12	5	6	7	
		October .	3	0	6	10	17	10	11	2	5	4	6	7	1	8	5	5	
		Sum..	19	10	20	22	29	19	24	15	26	32	29	42	27	32	21	22	S. 53.9° W.
50–55	10– 5	January.	6	4	0	1	5	0	6	11	20	8	8	10	8	4	5	4	
		April....	18	11	4	3	12	0	0	4	4	3	5	4	7	6	12	6	
		July	0	2	3	0	14	2	5	6	3	9	8	8	6	12	6	10	
		October .	0	0	14	0	0	0	8	6	10	0	19	0	24	0	0	0	
		Sum..	24	17	21	4	31	2	19	27	37	20	40	22	45	22	23	20	S. 63.3° W.

Some of these directions differ from those of Table XIII more than was expected. The difference is probably to be ascribed to the small number of the observations. Since Table XIV is deduced from the greatest number of observations, I have employed it in the comparisons exhibited in the following table:

TABLE XV.—*Comparison of storm paths with wind directions.*

Longitude.	Latitude of storm tracks.	Direction of storm tracks.	Latitude of wind directions.	Direction of wind.	Difference of latitude.	Wind most northerly.
60°	46.9°	N. 67.0° E	42.5°	N. 79.3° W	4.4°	−33.7°
50	48.9	N. 63.7 E	42.5	S. 85.2 W	6.4	−21.5
40	51.3	N. 66.7 E	47.5	S. 61.6 W	3.8	+ 5.1
30	53.9	N. 72.2 E	47.5	S. 73.5 W	6.4	− 1.3
20	54.9	N. 81.5 E	50.0	S. 65.2 W	4.9	+16.3
10	55.5	N. 86.2 E	52.5	S. 58.6 W	3.0	+27.6

Column 1 shows the longitudes for which the comparisons are made; column 2 shows the latitude of the points to which the direction of the storm tracks correspond; column 3 shows the average direction of the storm tracks for the months of January, April, July, and October; column 4 shows the latitudes corresponding to the wind directions from Table XIV; column 5 shows the direction of the wind for the given latitudes and longitudes, according to the United States Hydrographic charts; column 6 shows the differences of latitude between the points to which the storm tracks correspond and those to which the wind directions correspond; column 7 shows the difference between the average direction of the wind and the average direction of the storm paths for the points of comparison.

47. It will be seen that there is an average difference of nearly five degrees between the latitudes of the points for which the wind directions are given and those to which the storm tracks correspond. I have endeavored to deduce from Table XIII the proper correction of the wind directions for this difference of latitude, but the corrections appear so questionable that I have made no use of them.

We see that for the middle of the Atlantic Ocean, near the parallel of 50°, the average direction of storm paths corresponds very closely with that of the average progress of the wind; but in the western part of the Atlantic the average course of storms is considerably more northerly than that of the wind, while in the eastern part it is more southerly. These results accord very well with those derived from tropical storms, and seem to indicate that in the middle latitudes of the northern hemisphere the direction of progress of storm centers is not identically the same as that of the average wind, but is sensibly affected by some other cause or causes; and the results derived from observations in the China Sea seem to indicate that one of these causes is the prevalent direction of the wind which follows immediately after a storm.

48. An examination of the maps accompanying the monthly weather review of the United States Signal Service, which exhibit the tracks of storm centers, shows that between the Rocky Mountains and the meridian of 90° from Greenwich storms frequently travel towards the southeast. I have made a careful examination of the observations from this region, to determine whether the storm tracks of this region conform to the average direction of the winds. Table XVI shows the wind observations for twelve of the Signal Service stations in this region, for the three winter months, for the ten years from 1873 to 1882. I have restricted the comparison to the winter months, because during this period the winds are stronger and the northerly motion is more decided than during the warmer months of the year. The table shows for each station the sum of the observations for the year compared, and also the resulting course computed from the observations.

49. I next measured with a protractor, for each of the twelve stations, the direction of the storm paths as delineated on the monthly maps of the Signal Service for the winter months, during a period of ten years, from 1875 to 1884. I have omitted the observations of the two or three preceding years, because the storm tracks of this region are not as well delineated for those years as for the subsequent years. Table XVII shows the average results of these measurements and also the results derived from Table XVI. The directions are all measured from the north point towards the east.

TABLE XVI.—*Wind observations for the winter months.*

North Platte, Lat. 41° 8′, Long. 100° 45′.

	N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
1873	......	......	......	......	......	......	......	......	
1874	......	......	......	......	......	......	......	......	
1875	18	61	23	28	35	28	8	32	
1876	15	49	45	39	15	31	6	15	
1877	19	47	110	11	10	15	22	22	
1878	28	98	43	13	22	21	16	23	
1879	27	112	50	9	17	7	19	26	
1880	22	123	33	17	19	28	15	16	
1881	56	67	43	10	34	20	17	18	
1882	24	69	69	14	35	19	14	14	
Sum	209	626	416	141	187	169	117	166	N. 59.0° W.

Fort Sully, Lat. 44° 39′, Long. 100° 40′.

	N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
1873	16	59	47	8	4	22	48	14	
1874	8	79	11	5	7	45	21	11	
1875	20	100	15	0	9	55	20	9	
1876	40	71	31	5	16	41	38	19	
1877	3	115	20	7	4	57	8	11	
1878	......	......	......	......	......	......	......	......	
1879	......	......	......	......	......	......	......	......	
1880	......	......	......	......	......	......	......	......	
1881	......	......	......	......	......	......	......	......	
1882	......	......	......	......	......	......	......	......	
Sum.	87	424	124	25	40	220	135	64	N. 25.8° W.

Bismarck, Lat. 46° 47′, Long. 100° 38′.

	N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
1873	......	......	......	......	......	......	......	......	
1874	......	......	......	......	......	......	......	......	
1875	104	38	9	15	19	20	24	28	
1876	25	103	12	9	16	26	32	38	
1877	13	88	7	19	12	34	10	27	
1878	19	65	32	16	22	48	32	20	
1879	20	79	45	9	14	23	32	14	
1880	42	75	15	16	17	38	36	13	
1881	18	99	21	5	4	27	29	14	
1882	3	97	18	7	17	31	58	8	
Sum.	244	644	159	96	121	247	253	162	N. 17.3° W.

Yankton, Lat. 42° 54′, Long. 97° 28′.

	N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
1873	......	......	......	......	......	......	......	...	
1874	15	91	22	39	15	33	18	13	
1875	20	84	29	23	23	26	12	27	
1876	16	98	16	33	24	28	23	17	
1877	21	80	31	44	21	25	14	26	
1878	28	74	24	34	22	34	22	18	
1879	21	108	27	33	19	28	12	16	
1880	25	74	26	31	16	20	9	10	
1881	72	17	16	18	15	11	18	60	
1882	23	58	36	60	21	21	12	9	
Sum	241	684	227	315	176	226	140	196	N. 58.4° W.

Pembina, Lat. 49° 0′, Long. 97° 5′.

	N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
1874	1	111	0	13	2	116	0	10	
1875	0	51	0	13	0	36	0	6	
1876	10	99	25	13	28	50	8	7	
1877	17	66	33	11	76	26	2	3	
1878	33	61	21	14	63	58	10	2	
1879	22	132	18	4	65	17	5	0	
1880	26	94	18	3	78	16	8	1	
1881	......	......	......	......	......	......	......	......	
1882	......	......	......	......	......	......	......	......	
Sum	109	614	115	71	312	319	33	29	S. 85.7° W.

Breckinridge, Lat. 46° 11′, Long. 96° 17′.

	N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
1873	38	84	28	9	12	84	3	7	
1874	36	55	29	9	13	97	6	2	
1875	19	121	9	18	10	66	1	5	
1876	33	88	31	11	9	67	13	11	
1877	29	66	32	17	7	68	13	3	
1878	74	23	32	12	56	55	6	12	
1879	60	64	60	9	18	42	7	8	
1880	80	43	39	6	50	34	5	3	
1881	......	......	......	......	......	......	......	......	
1882	......	......	......	......	......	......	......	......	
Sum	369	544	260	91	175	513	54	51	N. 53.8° W.

Omaha, Lat. 41° 16′, Long. 95° 56′.

N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
68	54	13	25	32	13	4	6	
38	55	18	14	63	17	10	11	
58	67	13	16	56	20	10	14	
46	74	18	28	52	20	11	6	
62	52	18	31	52	14	10	8	
81	50	10	11	71	21	8	3	
32	87	38	17	37	20	11	15	
37	78	20	30	39	40	11	12	
70	69	9	19	55	23	10	8	
60	39	16	40	72	24	9	8	
552	625	173	231	529	212	94	91	N. 65.3° W.

Saint Paul, Lat. 44° 58′, Long. 93° 3′.

N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
18	41	43	32	6	30	26	6	
14	50	28	35	29	65	33	2	
14	55	54	52	25	31	17	4	
14	69	33	22	18	59	19	18	
33	46	40	50	19	49	9	6	
56	38	14	16	21	51	27	13	
21	83	41	45	14	43	8	6	
21	60	40	27	23	60	21	4	
25	60	33	21	17	46	23	8	
15	40	45	43	28	75	12	7	
231	542	371	343	200	509	195	74	S. 78.0° W.

Keokuk, Lat. 40° 22′, Long. 91° 26′.

N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
25	45	51	27	28	12	22	18	
25	49	39	33	37	27	21	21	
41	58	25	39	23	28	23	17	
29	64	29	32	42	32	21	9	
56	53	43	41	19	8	16	23	
47	37	25	35	40	23	17	35	
28	77	55	27	29	18	13	21	
37	55	31	45	37	31	14	14	
40	51	42	21	24	28	28	23	
18	47	40	44	43	32	21	17	
346	536	380	344	322	239	196	198	N. 75.3° W.

La Crosse, Lat. 43° 49′, Long. 91° 15′.

N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
23	44	16	45	42	52	2	34	
50	44	19	31	65	22	7	29	
51	32	57	33	68	11	4	10	
27	39	40	28	56	10	45	10	
59	21	40	37	75	10	3	6	
64	24	16	12	93	10	21	21	
60	47	44	31	60	4	4	16	
45	31	26	24	69	12	3	12	
66	34	31	25	59	19	14	8	
27	31	37	20	103	18	3	6	
472	347	326	286	690	168	106	152	S. 67.1° W.

Dubuque, Lat. 42° 30′, Long. 90° 44′.

N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
30	40	17	26	26	14	9	15	
19	42	57	40	27	28	12	12	
21	55	56	23	45	37	12	10	
22	52	28	25	13	9	3	14	
38	36	10	23	50	14	25	44	
44	83	27	20	24	15	6	14	
20	49	50	24	41	41	13	14	
26	57	40	20	31	22	22	12	
11	52	42	35	76	26	7	16	
231	466	327	236	333	206	109	151	N. 87.3° W.

Davenport, Lat. 41° 30′, Long. 90° 38′.

N.	NW.	W.	SW.	S.	SE.	E.	NE.	Course.
14	65	39	50	13	17	7	33	
16	49	51	41	23	16	28	27	
31	45	62	32	20	18	29	13	
7	72	43	46	22	24	34	13	
19	47	37	53	11	7	15	30	
24	30	25	47	28	16	19	47	
20	59	59	42	27	18	22	20	
19	47	38	48	35	18	27	21	
17	56	53	26	19	17	33	32	
16	55	44	67	28	21	18	21	
183	525	451	452	226	172	232	257	N. 83.2° W.

TABLE XVII.—*Comparison of storm paths with wind directions.*

Station.	Wind blows towards—	Storms move towards—	Storms most northerly.	Station.	Wind blows towards—	Storms move towards—	Storms most northerly.
North Platte......	N. 121.0° E.	N. 104.1° E.	+16.9	Omaha	N. 114.7° E.	N. 100.8° E.	+13.9°
Fort Sully	154.2	106.7	+47.5	Saint Paul........	78.0	99.3	−21.3
Bismarck	162.7	107.7	+55.0	Keokuk	104.7	88.3	+16.4
Yankton.....	121.6	106.3	+15.3	La Crosse.........	67.1	90.5	−23.4
Pembina	85.7	109.4	−23.7	Dubuque	92.7	88.3	+ 4.4
Breckenridge	126.2	105.5	+20.7	Davenport........	96.8	87.0	+ 9.8

50. From this table it is seen that at all of the stations (except Pembina, Saint Paul, and La Crosse) the average wind of winter blows towards a point somewhat south of east, and at the more western stations this direction is more than 30° south of east. It will also be seen that at the most western stations the average movement of storm centers is towards a point more than 15° south of east, but at the most eastern stations the direction is a little north of east. Comparing these numbers, we find that at the most western stations the average course of storm centers is from 10° to 20° more northerly than the course of the wind, while at the three stations above mentioned the average course of storm centers is decidedly less northerly than the course of the wind. The main result of this comparison is similar to that derived from observations on the Atlantic Ocean, viz, that there is not a rigorous correspondence between the average direction of the movement of storm centers and the progress of the wind, but that in some regions the average course of storm centers is more northerly than that of the wind, and in some regions it is more southerly.

51. It frequently happens that the southward motion of storm centers, which is shown by the preceding table in the valley of the Missouri River, is much more decided, and extends to much lower latitudes. Every year cases occur in which this southward motion extends to the parallel of 30°, and occasionally it extends to the parallel of 25°. Table XVIII shows cases in which storms have advanced towards the southeast as far as the parallel of 28°. The arrangement is similar to that of Table VIII. The first six columns describe each storm as long as its course continued southeasterly; the seventh column shows the highest pressure prevailing at any place within the limits of the United States on the north side of the low area, and the last column gives some indication of the subsequent course of each storm. The tracks of these storms are all delineated on Plate XIII, and are designated by the same numbers as in the table.

52. We see from this table that the average velocity of these storms while pursuing their course towards the southeast was 25 miles per hour, which is somewhat less than the average velocity of storms for the United States. In forming this average I have omitted No. 18, whose path is very imperfectly known. The lowest latitude attained by any of these storms was 23°, and in only four cases did the low center reach the parallel of 25°. In thirteen cases the storm center, after completing its course towards the southeast, changed its course and proceeded towards the north or northeast. In six of the remaining cases the intensity of the storm declined in advancing southward, and they apparently became extinct soon after the dates given in the table. The same was probably true in the four remaining cases, but the observations are not sufficient to establish this with certainty.

The low area, No. 12, was quite peculiar, having pursued a path almost directly opposite to that of ordinary storms. During the afternoon of August 20, 1878, there was an area of low pressure (29.75) over West Virginia, being part of a greater depression, whose center was over Newfoundland, and there was a slight tendency to the formation of an independent system of circulating winds. Owing to a slight increase of pressure on the north side the center of this low area was crowded southward, and in the afternoon of August 21 the low area (29.78) was pretty distinctly marked, and showed a feeble system of circulating winds. At 7.35 A. M., August 22, this low center had been crowded south to latitude 30°, the greatest observed depression being now 29.88. By the evening of August 22 the pressure at the center had increased to 29.95, and after this the low

TABLE XVIII.—*American storms advancing southeasterly.*

No.	Date.	Latitude. Beg. End.	Longitude. Beg. End.	Course.	Velocity.	Barometer on north.	Subsequent course.
	1874.				*Miles.*		
1	Feb. 17.2–18.2	33–27	86– 79	SE.	21.8	30.45	Became extinct
2	Apr. 13.3–16.3	41–26	102– 89	SE.	21.1	30.36	Unknown.
	1875.						
3	Jan. 14.3–16.2	44–27	106– 91	SE.	27.1	30.79	Became extinct.
	1876.						
4	Feb. 3.1– 4.1	33–28	98– 80	SE.	28.4	30.60	Became extinct.
5	Mar. 8.3–12.1	40–27	97– 89	SE.	15.7	30.66	Became extinct.
6	May 6.3– 7.3	33–27	100– 93	SE.	25.0	30.13	Unknown.
	1877.						
7	Jan. 4.2– 5.3	45–28	100– 90	SSE.	40.4	30.02	Northeast.
8	Mar. 21.2–24.1	42–28	100– 94	SSE.	22.5	30.48	Northeast.
9	Dec. 16 –20	41–28	126– 96	SE.	10.0	30.15	North.
10	Dec. 22 –27.2	47–27	126–101	SE.	29.7	30.35	Northeast.
	1878.						
11	Feb. 1.1– 2.3	33–26	96– 84	SE.	18.3	30.54	Northeast.
12	Aug. 20.2–24.2	38–23	83– 81	SSE.	15.1	30.14	Became extinct.
13	Nov. 16.2–17.2	28–24	102– 93	SE.	24.0	30.23	Northeast.
	1879.						
14	Jan. 6.3– 7.3	38–27	110– 98	SE.	39.2	30.41	Northeast.
15	Jan. 8.3–11.1	49–27	119– 98	SE.	30.4	30.25	Northeast.
16	May 4.1– 6.1	34–24	101– 96	SSE.	16.1	30.40	Unknown.
	1880.						
17	Mar. 7.3–13.1	45–28	109– 87	SE.	24.9	30.83	Became extinct.
18	Dec. 27.1–28.3	45–26	126– 92	SE.	57.5	30.63	Northeast.
	1881.						
19	Jan. 13.3–17.1	50–24	111– 97	SE.	28.0	30.65	Northeast.
	1882.						
20	Apr. 9.1–13.2	45–27	123– 83	SE.	24.9	30.54	Unknown.
	1883.						
21	Jan. 16.3–19.1	48–27	118– 96	SE.	32.3	30.79	Northeast.
	1884.						
22	Jan. 5.3– 7.2	45–29	120– 89	SE.	45.2	30.62	North northeast.
23	Mar. 14.3–18.1	37–27	124– 95	ESE.	22.2	30.42	Northeast.

center could not be distinctly traced. This example appears to illustrate the general character of areas of low pressure, and shows that this progressive movement is not due to a simple drifting of the atmosphere, but rather to a diminution of pressure on one side of the low area and an increase of pressure on the other side. In the present case there was scarcely any appreciable diminution of pressure on the south side, and only a slight increase of pressure on the north side.

The low area, No. 17, near the end of its course, exhibited similar peculiarities, and they were the result of similar causes. On March 12, at 3 P. M., there was a well marked low center (29.76) in the northwest part of Georgia, and a high center (30.83) existed in Dakota. At 11 P. M. this high area had advanced eastward, the low center had been crowded southward, and the pressure at the low center was 30.04. The next morning the pressure throughout Georgia and Florida had further increased, and the low center was crowded still further towards the southwest.

It will be seen from column 7 of the table that in all of the cases except four or five there was an area of decidedly high pressure on the north side of the low area, and in several of the cases the influence of this high area was similar to that already noticed in Nos. 12 and 17. The low area was crowded southward, and the depression gradually closed up, and became nearly or quite extinct. In some of the remaining cases in which the depression did not close up the high pressure on the north side was apparently the cause which crowded the low area so far to the southward.

53. The low areas enumerated in the preceding table were generally followed by a strong wind from the north or northwest. This will be seen from the following table, which shows the height of the barometer, with the direction and force of the wind, at five stations in several cases in which the low centers passed nearest to Galveston, in Texas.

TABLE XIX.—*Areas of low pressure, Nos. 2, 5, 8, and 20.*

	Leavenworth.		Fort Gibson.		Shreveport.		Galveston.		Indianola.	
	Barom.	Wind.	Barom.	Wind.	Barom.	Wind.	Barom.	Wind.	Barom.	Wind.
No. 2.—1874.										
Apr. 15. 1	30. 07	N. 10	29. 81	NE. 12	29. 83	S. 3	29. 83	SE. 9	29. 83	S. 20
15. 2	29. 93	NE. 12	. 72	NE. 13	. 77	NE. 11	. 70	S. 12	. 72	S. 22
15. 3	29. 97	NE. 10	. 73	N. 9	. 80	S. 2	. 75	S. 8	. 76	S. 10
16. 1	30. 05	E. 16	. 89	N. 8	. 84	Calm.	. 71	SE. 13	. 72	SE. 12
16. 2	30. 06	N. 12	. 93	NW. 13	. 81	NW. 6	. 64	N. 30	. 76	N. 26
16. 3	30. 16	Calm.	30. 10	N. 10	. 96	NW. 8	. 87	NE. 12	. 85	NE. 30
17. 1	30. 25	Calm.	. 20	N. 4	30. 05	NE. 10	. 94	NE. 22	30. 05	N. 40
No. 5.—1876.										
Mar. 10. 1	29. 64	N. 8	29. 51	S. 11	29. 82	S. 10	29. 83	SE. 4	29. 81	SE. 10
10. 2	. 67	NW. 15	. 46	S. 22	. 79	S. 9	. 78	SE. 6	. 73	SE. 18
10. 3	. 90	NW. 24	. 78	N. 14	. 76	S. 14	. 80	SE. 8	. 76	S. 22
11. 1	30. 14	NW. 16	. 95	N. 19	. 80	SW. 4	. 78	SE. 8	. 73	SE. 18
11. 2	. 18	N. 15	30. 07	N. 13	. 78	NE. 4	. 73	S. 8	. 77	SW. 10
11. 3	. 29	N. 16	. 16	N. 6	30. 04	NW. 7	. 93	NW. 39	. 94	N. 36
12. 1	. 44	N. 14	. 45	N. 25	. 23	NW. 9	30. 14	N. 30	30. 19	N. 48
No. 8.—1877.										
Mar. 22. 3	29. 82	N. 20	29. 66	SE. 13	29. 96	S. 3	29. 96	SE. 6	29. 93	SE. 16
23. 1	30. 00	N. 14	. 73	S. 8	. 97	Calm.	. 93	S. 6	. 90	SE. 8
23. 2	. 07	N. 10	. 79	NW. 17	. 83	S. 8	. 86	SE. 11	. 84	SE. 17
23. 3	. 23	N. 17	. 97	N. 16	. 94	S. 11	. 91	SE. 1	. 83	SE. 8
24. 1	. 34	N. 13	30. 18	N. 16	. 98	NW. 12	. 88	W. 20	. 99	N. 29
24. 2	. 23	N. 10	. 17	N. 18	30. 06	NW. 11	30. 02	NW. 38	30. 10	NW. 34
No. 20.—1882.										
Apr. 11. 1	30. 12	E. 13	29. 88	E. 10	29. 87	NE. 8	29. 80	S. 12	29. 76	SE. 11
11. 2	. 04	E. 16	. 81	NE. 16	. 74	E. 11	. 77	S. 18	. 73	SW. 30
11. 3	. 08	NE. 7	. 89	E. 8	. 80	E. 12	. 72	SW. 8	. 73	SE. 9
12. 1	. 12	E. 6	. 94	NE. 4	. 80	NE. 8	. 72	SE. 12	. 71	SE. 9
12. 2	. 09	NE. 11	. 95	N. 6	. 78	NE. 13	. 67	S. 16	. 62	SW. 23
12. 3	. 19	N. 7	30. 05	N. 14	. 85	N. 8	. 76	N. 16	. 77	N. 28
13. 1	. 19	N. 6	30. 12	N. 6	. 97	N. 12	. 84	N. 18	. 87	NE. 22
13. 2	. 17	N. 8	. 13	N. 8	30. 04	N. 9	. 90	NE. 18	. 92	N. 27
13. 3	. 23	N. 4	. 13	N. 4	. 09	N. 7	95	NE. 22	30. 04	N. 36
14. 1	. 26	NW. 4	. 22	N. 7	. 10	N. 8	30. 05	N. 24	. 08	N. 39

54. It sometimes happens that storms originating within the torrid zone, or within two or three degrees of it, and south of the United States, pursue a course of nearly 1,000 miles almost directly towards the north, while others pursue a very direct course towards the northeast. Table XX shows cases in which storms have traveled northward and eastward, and came from a point as far south as latitude 26°. The arrangement of the table is similar to that of Table XVIII. Columns 3 and 4 show the position of the storm center at the beginning and end of the northeasterly motion, as far as is indicated by the observations; column 5 shows the prevalent direction of the storm's progress; column 6 shows the average velocity of its progress in miles per hour; column 7 shows the lowest pressure reported, and column 8 gives a brief indication of the previous course of the storm. On Plate XIV these tracks are delineated, and are designated by the same numbers as in the table.

TABLE XX.—*American storms advancing northerly and easterly.*

No.	Date.	Latitude. Beg. End.	Longitude. Beg. End.	Course.	Velocity.	Lowest barometer.	Previous course.
	1872.				*Miles.*		
1	Nov. 6.1– 7.3	26°–47°	95°–55°	ENE.	60.4	29.71	Unknown.
2	Nov. 7.3– 9.3	25 –30	95 –78	ENE.	21.1	29.74	Unknown.
3	Dec. 9.2–13.3	26 –47	101 –57	NE.	28.6	29.86	Unknown.
4	Dec. 23.2–27.2	25 –44	95 –58	NE.	29.8	29.17	Unknown.
	1873.						
5	Feb. 19.1–22.1	21 –45	98 –64	NE.	35.1	29.17	Unknown.
6	May 4.1–10.1	24 –43	98 –81	NE.	15.8	29.57	Unknown.
7	Sept. 18.1–20.1	24 –34	92 –74	NE.	24.3	29.57	Unknown.
8	Sept. 22.3–24.1	25 –36	86 –72	NE.	28.5	29.78	Unknown.
9	Oct. 5.1– 8.2	25 –43	87 –62	NE.	32.9	29.02	Towards NW.
10	Dec. 24.2–27.1	24 –43	88 –62	NE.	30.4	29.37	Unknown.
	1874.						
11	Jan. 5.2– 9.1	25 –49	87 –68	NNE.	18.0	29.42	Unknown.
12	Feb. 7.2–11.1	25 –46	82 –58	NNE.	25.0	28.95	Towards NW.
13	Apr. 17.3–24.1	24 –46	94 –59	N. & NE.	29.7	29.36	Unknown.
14	Sept. 2.3-10.2	22 –50	99 –89	N.	21.5	29.47	Unknown.
15	Sept. 27.1–30.2	25 –50	87 –66	NNE.	26.0	28.94	Unknown.
16	Dec. 18.2–21.1	25 –39	96 –62	NE.	34.6	29.33	Unknown.
	1875.						
17	Nov. 6.1– 7.3	25 –31	98 –78	ENE.	32.9	29.82	Unknown.
	1876.						
18	Oct. 19.1–21.1	21 –32	82 –72	NNE.	19.5	29.51	Not traceable.
	1877.						
19	Sept. 16.1–21.3	25 –31	96 –76	ENE.	10.7	29.40	Unknown.
	1878.						
20	Jan. 6.1–12.2	24 –46	100 –56	NE.	26.4	28.85	Not traceable.
21	Feb. 26.2–28.1	24 –30	92 –71	ENE.	31.1	29.71	Came from NW.
22	Mar. 17.1–17.2	23 –25	85 –78	ENE.	(?)	29.79	Not traceable.
23	Mar. 19.3–22.3	25 –27	95 –78	E.	15.0	29.71	Came from W.
24	July 2.1– 2.3	25 –27	85 –78	ENE.	22.9	29.77	Not traceable.
25	Sept. 24. –33	15 –32	76 –61	N. & NE.	10.1	29.70	Not traceable.
26	Oct. 21.1–24.2	20 –38	81 –57	N. & E.	27.5	28.83	Not traceable.
27	Nov. 13.3–20.1	22 –14	97 –57	E. & NE.	24.5	29.83	Not traceable.
28	Nov. 17.2–21.1	24 –47	93 –57	NE.	40.3	29.47	Came from NW.
	1879.						
29	Nov. 19.1–20.3	23 –49	74 –60	NNE.	48.8	29.00	Not traceable.
	1880.						
30	Jan. 24. –28.1	21 –36	86 –75	N.	14.3	29.68	Not traceable.
31	Mar. 7.3– 9.2	26 –32	99 –74	ENE.	38.0	29.86	Not traceable.
32	May 3.1– 6.2	26 –47	93 –59	NE.	23.8	29.79	Unknown.
33	Aug. 19 –20	20 –27	78 –74	NNE.	12.4	29.86	Towards NW.

55. We see from this table that storms of this class occur most frequently in the autumn and least frequently in the summer. One of these storms began near latitude 15°, two began near latitude 20°, and seventeen of them began south of latitude 24°. Three of these storms had been traveling towards the northwest, previous to the dates given in the table, and two of them came from the northwest; but in the other cases the barometric depression was too small to allow us to trace their course previous to the dates here given. For most of the cases in the last half of the table this is clearly shown by the international observations, and we may therefore infer it to be true in the other cases. As long as these storms continued south of latitude 30° the barometric depression was generally small, but it increased as the storm advanced northward. In fifteen cases the barometer fell below 29.5 inches, and in four cases it fell below 29.0 inches. The average velocity of progress of these storm centers, while advancing northward and eastward, was 26.9 miles per hour. From a comparison of this table with Table III we perceive that the American storms which originate between the equator and latitude 20° N., generally travel towards a point between north and west, but occasionally they advance almost exactly northward.

56. In order to institute a comparison between the peculiarities of these storms which travel northward and those which travel almost exactly towards the south I have selected those storms which passed near Galveston, Tex., and which pursued paths almost exactly opposite to those shown in Table XVIII. Table XXI exhibits the leading phenomena of these storms, as determined by observations at five stations.

TABLE XXI.—*Areas of low pressure, Nos. 6, 13, 14, and 27.*

	Indianola.		Galveston.		Shreveport.		Fort Gibson.		Leavenworth.	
	Barom.	Wind.	Barom.	Wind.	Barom.	Wind.	Barom.	Wind.	Barom.	Wind.
1873—No. 6.										
May 4.3	29.89	E. 34	29.91	E. 13	29.96	E. 8	30.05	E. 2	29.98	Calm.
5.1	.83	E. 14	.87	SE. 12	30.00	NE. 6	.06	E. 2	30.00	Calm.
5.2	.70	SW. 17	.67	SE. 18	29.75	NE. 13	29.96	NE. 11	29.87	S. 3
5.3	.79	E. 10	.77	NW. 3	.73	SE. 13	.87	NE. 12	.90	E. 2
6.1	.85	W. 6	.78	NW. 12	.72	SW. 11	.85	E. 4	.91	Calm.
6.2	.82	N. 4	.76	W. 13	.70	SW. 8	.78	NE. 7	.87	SE. 12
6.3	.91	N. 5	.85	NW. 4	.76	W. 5	.83	N. 6	.78	SE. 13
7.1	.96	SW. 1.	.92	SW. 4	.94	W. 6	.90	W. 1	.77	N 5.
1874—No. 13.										
Apr. 18.1	29.85	NE. 11.	29.84	E. 7	30.05	NE. 5	30.14	E. 8	30.22	Calm.
18.2	.79	N. 15.	.87	SE. 8	29.87	NE. 8	.09	SE. 9	.10	SE. 5
18.3	.84	N. 22.	.78	SE. 3	.82	NE. 6	29.93	NE. 12	.09	SE. 1
19.1	.91	NW. 14	.82	NW. 20	.70	SE. 6	.67	E. 4	29.92	E. 15
19.2	.89	W. 20	.81	W. 18	.70	SW. 13	.47	S. 20	.50	E. 24
19.3	.98	W. 4.	.94	SW. 5	.92	S. 8	.46	S. 28	.39	NE. 7
20.1	30.09	SW. 7	30.03	SW. 11	.99	SW. 7	.80	W. 20	.61	NW. 20
1871—No. 14.										
Sept. 5.1	29.85	NE. 38	29.89	NE. 16	29.95	E. 4	29.99	Calm	29.98	S. 3
5.2	.78	E. 48	.84	E. 17	.88	E. 1	.89	NW. 3	.85	S. 10
5.3	.81	SE. 36	.89	SE. 10	.96	S. 8	.93	E. 5	.89	S. 6
6.1	.87	SE. 18	.91	E. 12	.97	E. 4	30.00	SE. 6	.97	S. 1
6.2	.87	SE. 20	.89	SE. 15	.92	E. 2	29.92	S. 14	.86	S. 10
6.3	.91	SE. 18	.95	SE. 12	.98	E. 4	.99	SE. 6	.94	S. 16
7.1	.94	SE. 12	.95	SE. 8	.98	Calm.	30.03	SE. 5	30.02	Calm.
1878—No. 27.										
Nov. 14.1	29.94	NE. 24	29.97	SE. 12	30.08	Calm.	30.08	E. 9	30.19	E. 4
14.2	.88	NE. 15	.92	E. 15	29.98	E. 8	29.99	E. 6	.16	SE. 4
14.3	.91	NW. 10	.91	NE. 8	.96	Calm.	30.05	S. 8	.09	E. 3
15.1	30.01	NW. 12	.95	W. 8	.92	Calm.	29.94	N. 3	.04	E. 1
15.2	29.96	N. 10	.97	NW. 11	.92	W. 1	.83	W. 8	29.89	S. 1
15.3	.99	E. 5	.99	N. 4	.98	Calm.	.90	W. 3	.91	S. 2
16.1	.95	SE. 9	.98	E. 5	30.01	Calm.	.98	Calm.	.97	N. 4

By comparing Table XXI with Table XIX, we perceive that in the case of the storms which traveled northward the average barometric oscillation was only two-thirds of that of the storms which traveled southward: that the average force of the wind in the former case was less than two-thirds of that in the latter case; that in the latter case the winds which followed the low center were generally from the north, and that 92 per cent. of them were either from the north, northwest, or northeast, while in the former case the winds were very irregular, but southerly winds were somewhat more frequent than northerly winds. We perceive, then, that the progress of the storms which traveled southward conformed closely to that of the winds which succeeded the low center, but that in the case of the storms which traveled northward, although there was some tendency towards the same law, this tendency was not strongly marked, and the winds were generally moderative in force. It seems then probable that some other cause or causes than that of the prevalent wind exerted an appreciable influence in diverting these areas of low pressure towards the north. Among these causes may be mentioned the influence of neighboring areas of high and low pressure, as will be shown hereafter.

57. Occasionally we find instances in which the storms of the middle latitudes of the United States pursue a course still more abnormal than those shown in Tables XVIII and XX. The Signal Service maps show cases in which the storms of the middle latitudes have pursued for a short time a westerly course. In some of these cases the depression of the barometer was small and there was no well-defined storm center. Sometimes there were two centers of slight depression within a few hundred miles of each other, so that a small change in the force of the winds caused one of the centers to predominate slightly, and thus the center of greatest depression was carried in an unusual direction. Table XXII shows the most decided cases in which the centers of low pressure in the middle latitudes of the United States have advanced in a westerly direction. Column 1 gives the reference number; column 2 the date of beginning and end of the westerly

movement; column 3 the duration of the westerly movement, expressed in units of eight hours; columns 4 and 5 the latitude and longitude of the points of beginning and end of the westerly movement; column 6 the prevalent direction of this movement; column 7 its velocity in miles per hour; column 8 the lowest barometer reported during the continuance of this westerly movement; and column 9 shows its subsequent course. These abnormal movements are all delineated on Plate XV, and are designated by the same numbers as in the table.

TABLE XXII.—*Areas of low pressure in the United States, advancing westerly.*

No.	Date.	Duration.	Latitude. Beg. End.	Longitude. Beg. End.	Course.	Velocity.	Lowest barometer.	Subsequent course.
	1873.					*Miles.*		
1	Oct. 20.1–21.3	5	38°–45°	75°– 86°	NW.	20.7	29.26	NE.
	1874.							
2	May 9.1– 9.3	2	49 –43	98 –103	SW.	37.3	29.01	NE.
	1876.							
3	Jan. 8.3– 9.1	1	44 –43	84 – 86	SW.	22.2	29.43	ENE.
4	Feb. 25.3–26.3	3	42 –38	95 – 97	SSW.	8.5	29.40	Eastward.
5	June 17.3–18.2	2	44 –47	86 – 89	NNW.	12.7	29.37	Southerly.
6	Sept. 17.2–17.3	1	38 –42	77 – 80	NNW.	28.8	29.16	E.
	1877.							
7	Feb. 21.2–22.1	2	48 –43	89 – 92	SSW.	33.9	29.35	Eastward.
8	Nov. 22.3–24.3	6	32 –42	79 – 84	NNW.	15.7	29.63	Coalesced.
	1878.							
9	Feb. 19.2–20.1	2	43 –35	95 – 98	SSW.	24.6	29.33	NE.
10	Mar. 10.1–11.1	3	43 –44	96 –102	WNW.	18.0	29.47	Eastward.
11	Mar. 23.1–24.1	3	50 –42	57½– 72	SW.	36.0	29.22	ENE.
12	Apr. 28.2–29.1	2	37 –41	77 – 80	NNW.	18.5	29.58	Coalesced.
13	June 22.2–23.1	2	42 –44	76 – 79	NNW.	15.7	29.61	ENE.
14	Nov. 5.1– 9.2	13	49 –47	58 – 68	W. by S.	15.0	29.18	Coalesced.
	1879.							
15	July 12.1–12.3	2	39 –34	73 – 80	SW.	35.5	29.57	Became extinct.
16	Dec. 27.1–27.3	2	45 –42	93 – 98	SW.	25.0	29.69	ENE.
	1880.							
17	Feb. 26.2–27.1	2	42 –38	94 –100	SW.	26.9	29.51	ENE.
18	Sept. 17.1–17.3	2	46 –41	95 – 98	SSW.	17.2	29.61	NNE.
19	Oct. 28.1–28.3	2	49 –44	97 –102	SW.	24.1	29.77	Coalesced.
	1881.							
20	May 18.2–20.1	5	38 –46	71 – 78	NW.	17.3	29.82	Disappeared.
21	Aug. 27.2–30.2	9	31 –45	80 – 95	NW.	15.4	29.68	Disappeared.
22	Sept. 15.2–17.2	6	38 –50	87 – 97	NNW.	21.5	29.43	Unknown.
23	Dec. 12.2–12.3	1	47 –42	91 – 93	SSW.	35.5	29.69	ENE.
	1882.							
24	Mar. 25.3–26.1	1	42½–39	93 – 99	SW.	28.7	29.58	ENE.
25	Apr. 11.3–13.1	4	45 –49	62 – 68	NW.	17.0	29.21	ENE.
26	July 5.3– 6.1	1	42 –45	70 – 74	NW.	28.7	29.78	NE.
	1884.							
27	Mar. 5.2– 7.1	5	35 –29	93 – 98	SW.	12.5	29.80	ENE.
28	May 19.3–20.1	1	44 –39	97 –104	SW.	43.1	29.62	ENE.
29	June 10.2–13.1	8	39 –32	80 – 86	SSW.	14.4	29.68	Became extinct.

58. We perceive that these cases are distributed with tolerable uniformity through the different months of the year; they are not restricted to any particular portion of the United States, and their average velocity of progress is 23 miles per hour, which is somewhat less than the average velocity of the storms of the United States. If we seek in each case for the probable cause of its abnormal movement we shall be forced to conclude that this cause was not the same in all cases. Among these causes we notice the following:

I. Sometimes near to an area of low pressure, with its system of circulating winds, we find a second area of low pressure, having also its own system of circulating winds. Between these two low centers the winds are usually feeble, and they sometimes change into a single system of winds circulating about a low area of an elongated form. If the depression continues the area of low pressure usually becomes less elongated and the two low centers coalesce. By this union the western low center is accelerated eastward, and the eastern low center is temporarily diverted towards the west. Table XXII affords several illustrations of this principle.

No. 8 was at first a small depression near Charleston, S. C., and was advancing slowly north-

ward, while a greater depression prevailed at the same time in Dakota, and was advancing eastward. These two centers thus approached each other and became partially blended on the 24th, but did not form a grand depression, probably on account of a general low pressure, which prevailed at that time over a large portion of the United States, and which resulted in feeble gradients.

No. 14 continued its abnormal movements for an unusually long period, but the time given in the table embraces two periods of movement towards the west, separated by a period in which its motion was eastward, and its abnormal movements were only in part due to the cause here considered. November 6 there was a second area of low pressure in the valley of the Mississippi, and this low center partially controlled the winds to a distance of nearly 1,000 miles on its eastern side. This second low was apparently one of the causes which attracted westward the low prevailing near the Gulf of Saint Lawrence. This second westerly movement of the latter low was apparently due in part to another low area, which prevailed at that time in the neighborhood of Hudson's Bay.

No. 19 was apparently diverted towards the southwest by a second low area, which advanced from California eastward, and which coalesced with the former on the evening of October 28.

No. 21 was similar to No. 8, being apparently diverted towards the northwest by a low area prevailing in Dakota, and with which it coalesced August 29.

No. 26 was a small low near the Atlantic coast, which was apparently attracted by a greater low prevailing in Minnesota, and with which it coalesced.

II. Sometimes a heavy fall of rain or snow appears to divert a system of circulating winds towards the region of rainfall, and the center of a low area may be thereby carried in an abnormal direction. Table XXII affords several illustrations of this principle. No. 1 was a storm of unusual violence, accompanied by very high winds, and a very unusual fall of rain, and the greatest amount of rain fell on the north or northwest side of the low center. Table XXIII shows the rainfall at twelve stations, as reported at each of the three daily observations for two days. The last column shows the aggregate fall for the entire period of forty eight hours. The center of the low area appeared to be attracted toward the region of greatest rainfall.

TABLE XXIII.—*Rainfall October 19.2 to 21.1, 1873.*

	19.2	19.3	20.1	20.2	20.3	21.1	Sum.
Norfolk	0.15	0.56	0.32	0.46	0.02	0.00	1.51
Lynchburg	.55	.65	.32	.11	.07	.00	1.70
Washington	.13	1.32	1.11	.43	.01	.08	3.08
Cape May	.00	.44	.77	.85	.31	.00	2.37
Baltimore	.22	.78	1.78	.86	.05	.02	3.71
Philadelphia	.00	.06	1.96	.21	.55	.00	2.78
Pittsburgh	.51	.51	.97	.32	.67	.33	3.31
New York	.00	.02	.75	.10	.03	.04	0.94
Buffalo	.31	.10	.32	.90	1.23	.05	2.91
Rochester	.31	.44	1.19	1.38	1.20	.49	5.01
Oswego	.17	.23	1.42	1.02	.57	.15	3.56
Kingston, Canada	.42	.21	.85	.30	.35	.00	2.13

No. 5 was a storm similar to the preceding, but less violent. Table XXIV shows the rainfall at seven stations during a period of two days. The greatest rainfall was generally on the northwest side of the low center.

TABLE XXIV.—*Rainfall June 16.3 to 18.2, 1876.*

	16.3	17.1	17.2	17.3	18.1	18.2	Sum.
Grand Haven	1.03	0.00	1.08	0.28	0.32	0.18	2.89
Milwaukee	.01	.38	.34	.12	.28	.03	1.16
La Crosse	.13	1.13	.92	.15	.43	.00	2.76
Alpena	.00	.02	.13	.00	.00	.00	0.15
Escanaba	.33	.37	.26	.39	.08	.20	1.63
Marquette	.19	.82	.03	.90	.04	.02	2.00
Duluth	.50	.00	.51	.44	.27	.00	1.72

No. 6 was a storm similar to No. 1, with equally violent winds, and a somewhat greater fall of rain, and the greatest rainfall was on the north and northwest sides of the low center. Table XXV shows the rainfall at ten stations during a period of two days.

TABLE XXV.—*Rainfall September* 16.2 *to* 18.1, 1876.

	16.2	16.3	17.1	17.2	17.3	18.1	Sum.
Charleston	0.03	0.40	2.30	0.00	0.00	0.00	2.73
Wilmington	.57	.24	3.16	.48	.00	.00	4.45
Norfolk	.70	.62	.32	.66	.00	.00	2.30
Washington	.01	.10	.37	2.84	.05	.00	3.37
Cape May	.01	.28	1.46	.20	.02	.00	1.97
Baltimore	.03	.09	1.18	2.42	.42	.00	4.14
Philadelphia	.00	.01	1.38	1.68	.42	.00	3.49
New York	.00	.00	1.03	1.20	.40	.00	2.63
Pittsburgh	.00	.10	.30	1.01	2.07	1.00	4.48
Erie	.00	.00	.00	1.10	1.30	2.00	4.40

No. 11 was an area of low pressure which traveled eastward near the parallel of 50°, and when it had passed a little beyond Quebec was for a few hours diverted towards the southwest. This westward movement of the low center was accompanied by a considerable fall of snow in the neighborhood of Lakes Erie and Ontario. Table XXVI shows the amount of the precipitation (reduced to water) at five stations during a period of twenty-four hours.

TABLE XXVI.—*Fall of snow (reduced to water) March* 23.3 *to* 24.2, 1878.

	23.3 inch.	24.1 inch.	24.2 inch.	Sum. inch.
Cleveland	0.00	0.48	0.14	0.62
Erie	.00	.30	.05	.35
Buffalo	.15	.20	.58	.93
Rochester	.04	75	.17	.96
Oswego	.06	.23	.06	.35

No. 12. The northerly and westerly movement of this low area on the afternoon of April 28 was accompanied by a considerable rainfall on its northwest side, and there was also on the northwest side a second low area, which was traveling eastward, and which coalesced with the former on the evening of April 29. The latter cause may have promoted the westerly movement of No. 12.

No. 13 was similar to the preceding. The movement of this low center towards the northwest on the afternoon of June 22 was accompanied by considerable rain on the northwest side of the low center, and there was also on the northwest side a second low area which coalesced with the former on the following day.

No. 14. There was a heavy fall of snow in the neighborhood of the Gulf of Saint Lawrence, in connection with this storm, and there are indications that this snow-fall had some influence upon the movements of this low area, but, as I have no observations except those published in the International Bulletin, and as these are given but once a day, the evidence is not entirely satisfactory.

No. 24. The diversion of this low center towards the southwest on the afternoon of March 25 was apparently the result of a heavy fall of rain in Texas.

III. When two centers of high pressure are situated within a few hundred miles of each other a feeble system of circulating winds frequently springs up between them, and if there is a considerable fall of rain or snow a new center of low pressure is usually formed, and this may occasion abnormal movements in a neighboring area of low pressure, in the manner described in Paragraph I.

No. 20 affords an illustration of this principle. There was a center of low pressure over North Carolina, with a center of high pressure over Newfoundland, and another over Montana. Between

these two areas of high pressure the winds were feeble and several centers of local disturbance were formed, attended by some rain. The barometer fell slowly, and the low center over North Carolina was carried northward and somewhat westward.

No. 22 was similar to the preceding, except that the rain-fall was very great, and the low center moved rapidly northward, and the depression at the center of the low area increased.

The movement of No. 23 towards the southwest, December 12, was apparently due to a similar cause.

No. 25 may probably be ascribed in part to the same cause. There was a center of low pressure near Nova Scotia, with a high area near Lake Superior, and a second high area over Greenland. At the same time there was a considerable fall of snow on the north and northwest sides of the low center, and by the joint influence of these two causes the low center was carried northward and westward.

The first movement of No. 14 towards the northwest was apparently due to a similar cause.

IV. Sometimes we find an extensive area of low pressure with feeble gradients on its southern side, having northerly winds on its north side and southerly winds on its south side. On the north side the barometer rises and the thermometer falls; on the south side the opposite effects usually take place, but in a less marked degree, and occasionally on the soath side the barometer does not fall at all. In either case the center of low pressure is diverted towards the south or southwest, even when no rain-fall, or only a fall of two or three hundredths of an inch, is reported on that side of the low center. This abnormal movement of the low center appears generally to result from the influence of an area of high pressure (or relatively high pressure) prevailing on the north or northeast side, and crowding southward with considerable force. The center of the low area appears to be displaced by the influence of the high area, which fills up the low area on its northern side, and generally but little change takes place on its southern side. Table XXII affords several examples of this kind. Table XXVII presents a summary of the leading particulars relating to twelve of these low areas. Column 1 gives the number taken from Table XXII; column 2 shows the change of the barometer in twenty-four hours on the northern side of the low center; column 3 shows the change in the thermometer on the northern side during the same period; column 4 gives the barometric gradient on the northern side, showing (in decimals of an inch) the change of pressure for a distance of 60 nautical miles; column 5 shows the prevalent wind on the north side; and column 6 shows the magnitude of the high area prevailing within a few hundred miles on the north side of the low center; columns 7–10 show corresponding particulars for the south side of the low center; column 11 shows whether any rainfall was reported on the south side of the low center; and column 12 shows the change of pressure, which took place at the low center during the period of the western motion.

TABLE XXVII.—*Changes in areas of low pressure.*

No.	On the north side. Change in 24 hours. Barom.	Change in 24 hours. Therm.	Barometer gradient.	Prevalent wind.	High.	On the south side. Change in 24 hours. Barom.	Change in 24 hours. Therm.	Barometer gradient.	Prevalent wind.	Rain.	Change at low center.
	Inch.		*Inch.*		*Inches.*	*Inch.*		*Inch.*			*Inch.*
2	+0.25	−10	0.08	NE. & NW.	30.38	−0.09	0	0.06	S.	Slight......	0.00
3	+ .19	−28	.10	NE. & NW.	30.24	− .15	+10	.09	S. & SW.	Considerable	+ .10
4	+ .16	− 8	.14	N. & NE.	30.90	− .18	+ 8	.10	S.	Slight......	− .20
7	+ .57	−29	.08	N.	30.33	− .20	+ 6	.06	S. & SW.	Slight	+ .11
9	+ .34	− 5	.08	N. & NE.	30.00	− .51	+ 4	.05	S.	Rain in Texas	+ .07
15	+ .32	− 3	.05	N. & NE.	29.99	.00	0	.03	SW.	Slight......	+ .19
16	+ .20	− 2	.07	N. & NE.	30.35	− .33	+11	.06	S. & SE.	None.......	− .07
17	+ .33	−17	.16	N.	30.69	.00	0	.06	S.	None.......	+ .13
18	+ .11	− 2	.06	N. & NE.	30.12	− .11	+12	.05	S.	Slight......	+ .08
27	+ .20	− 8	.05	N. & NE.	30.41	− .08	+ 1	.04	S.	Slight......	+ .04
28	+ .34	−20	.03	N. & NW.	30.15	− .18	+ 6	.04	S.	Slight......	+ .13
29	+ .13	0	.03	NE.	30.43	− .03	0	.03	SW.	Slight......	+ .24

In No. 16 there was a second low area advancing from the northwest, with which No. 16 coalesced December 28. No. 17 was similar to the preceding. These two numbers may, therefore, perhaps be transferred to Class I.

No. 9 was attended by considerable fall of rain in Texas, and this case may perhaps be transferred to Class II.

No. 4 was attended by an area of high pressure on the east side and a second area of high pressure on the west side of the low center, and between these two high areas the isobars were very much elongated towards the southwest. This case may, therefore, perhaps be transferred to Class III.

The eight remaining numbers show a remarkable agreement in their general features. The depression at the center of the low area did not in either of these cases increase during the continuance of the westerly movement, and generally the low became decidedly more shallow.

No. 10 bears a decided resemblance to the cases in Class IV. There prevailed on March 10 a high area (30.40) on the Atlantic Coast; a second high (30.20) on the Pacific Coast; and a third high (30.60) near Hudson's Bay. The latter high area pushed downward towards the southeast, and this appeared to be the cause which crowded towards the west the low center which existed near the Missouri River. There was some fall of rain and snow, but this alone does not seem to be sufficient to account for the abnormal movement of this low center.

59. I next sought for cases in which storms in the middle latitudes of the Atlantic Ocean or Europe have for a day or more pursued a westerly course. For this purpose I have carefully consulted Hoffmeyer's daily weather charts from December, 1873, to November, 1876; also the daily charts of the Danish Meteorological Institute and the Deutsche Seewarte from December, 1880, to August, 1881; also the monthly maps of storm tracks published by the Deutsche Seewarte from 1876 to 1884, and the monthly maps of storm tracks according to the international observations from November, 1877, to April, 1882. Table XXVIII shows the most decided cases of these westerly movements. These abnormal movements are represented on Plate XVI, and are designated by the same numbers as in the table.

60. We perceive from this table that these abnormal movements occur in all months of the year, but are most frequent in spring. We also perceive that movements towards the northwest are more than twice as frequent as those towards the southwest, whereas in the United States southerly movements are most common. The average velocity of progress of storm centers during their westerly movement is 15.5 miles per hour, which accords very closely with the average velocity of all storm centers for Europe.

If we carefully examine each case in the preceding table we shall find examples of three, if not all, of the classes already described for the United States. In nearly every case we find a fall of rain or snow in the region toward which the low center advanced, and in most of the cases the rainfall was unusually great. In the latter statement are included Nos. 6, 9, 15, 16, 17, 18, 19, 20, 22, 30, 34, 37. and 41. This list does not include cases in which the low center was over the Atlantic Ocean, since in these cases the amount of rainfall is unknown. It may be inferred from this comparison that a fall of rain or snow is one of the most important causes which determine the abnormal movements of areas of low pressure.

In a large number of the cases in Table XXVIII the low center appeared to be attracted towards a second low center which was at no great distance, and in several cases the two low centers subsequently coalesced. Nos. 1, 2, 3, 4, 5, 7, 8, 9, 11, 12, 13, 14, 18, 19, 20, 23, 24, 26, 28, 29, 30, 31, 34, 35, 37, 40, and 41 exhibit this attractive influence of a second low center, and in Nos. 1, 2, 5, 8, 11, 18, 23, 28, 29, 30, 34, 35, 40, and 41 the two low centers coalesced. The mutual influence of two low centers would thus seem to be even more efficient than that of rainfall, but these two causes generally concur; and the correspondence between the two lists of numbers here given would probably be more complete if the observations had furnished a full report of the rainfall in the neighborhood of each low center.

The number of cases in which the low center advanced between two areas of high pressure, not very remote from each other, or in a direction lying between two such areas and apparently under their influence, is very great. Nos. 1, 2, 3, 4, 6, 7, 10, 11, 14, 16, 17, 18, 19, 21, 23, 27, 29, 36,

TABLE XXVIII.—*Storms advancing westerly over Europe and the Atlantic Ocean.*

No.	Date.	Latitude. Beg. End.	Longitude. Beg. End.	Course.	Velocity.	Lowest barometer.	Subsequent course.
	1875.	°	°		*Miles.*		
1	Mar. 14–16	50 –46	35–41½ W.	SW.	9.2	730mm	Coalesced.
2	Dec. 17–19	64 –64	29–43 W.	W.	10.5	720	Easterly.
	1876.						
3	Apr. 19–20	52 –56	3–5 W.	NNW.	13.1	735	Northeast.
4	June 19–20	57½–60½	23½–27 W.	NW.	10.9	730	Subdivided.
5	June 22–23	57¼–59	26–33½ W.	WNW.	14.1	740	Disappeared.
6	Sept. 9–12	55 –60	21–7 E.	WNW.	9.6	735	Subdivided.
7	Sept. 22–23	54½–56	26½–30 W.	NW.	8.8	740	Subdivided.
8	Oct. 20–21	56¾–61	34½–49 W.	NW.	28.8	720	Eastward.
9	Dec. 21–23	53 –56	1–8 W.	WNW.	8.6	729	Southerly.
	1877.						
10	Apr. 4– 8	56 –50	9–13 W.	SSW.	5.0	733	East northeast.
11	May 1– 7	45 –65	48–16 E.	NW.	13.8	745	Disappeared.
12	July 15–16	54 –54	2–4½ W.	W.	4.2	737	Northeast.
13	Aug. 9–10	54 –59½	0–2 W.	NNW.	14.9	745	Coalesced.
	1878.						
14	Mar. 31–32	59½–58½	10 E.–0	W.	14.8	729	Eastward.
15	May 30–32	60 –59½	27½–17½ E.	W.	12.1	741	Northeast.
16	Nov. 4– 6	54 –59	25–19 E.	NW.	10.1	737	Northeast.
17	Nov. 11–13	59 –50	6–1 E.	SSW.	15.1	733	Eastward.
18	Nov. 14–16	44 –54	13–4 E.	NNW.	17.6	734	South.
19	Dec. 10–11	52 –56½	25–23 E.	NNW.	13.5	739	Northeast.
	1879.						
20	Feb. 20–21	53 –54	32–11½ E.	W.	35.0	735	Northeast.
21	May 7–12	62½–65	29½ E.–7 W.	W.	18.4	738	Coalesced.
22	May 27–28	50 –48	6 E.–4 W.	W.	18.8	747	Northeast.
23	May 31–32	60 –60	9–4 E.	W.	7.2	742	Divided.
24	Sept. 7– 8	55 –58	9–14 W.	NW.	12.2	735	Northeast.
	1880.						
25	July 21–22	58 –64	62–50 E.	NW.	24.4	739	Eastward.
26	Oct. 3– 4	60 –63	28–23 E.	NW.	10.9	735	Eastward.
	1881.						
27	Jan. 12–14	60 –71	42–36 E.	NNW.	16.2	746	Unknown.
28	Feb. 26–27	48½–46½	34–44½ W.	WSW.	18.6	745	Northward.
29	Mar. 3– 4	48 –49	24½–26 W.	NW.	5.0	735	Northward.
30	Aug. 17–18	59½–63	30½–22 E.	NW.	15.8	737	Uncertain.
	1882.						
31	Mar. 30–34	60 –50	5 E –20 W.	SW.	17.2	742	Coalesced.
32	May 24–26	58 –50	5–20 W.	SW.	17.5	738	Northward.
33	July 8– 9	57 –57	6–11 W.	W.	7.8	740	Northeast.
34	July 10–12	58 –62	19–17 E.	N.	6.5	735	Northeast.
35	Nov. 21–23	58 –60	16 E.–10 W.	W.	19.5	738	Northeast.
	1883.						
36	Jan. 6– 7	49 –55	45–40 E.	NNW.	19.4	749	Northeast.
37	Mar. 8– 9	46 –40	23–3 E.	WSW.	42.0	748	Northeast.
38	Mar. 14–18	57 –55	40 E.–6 W.	W.	19.3	742	Uncertain.
39	Mar. 21–23	45½–41	10–19 W.	WSW.	12.9	748	Northeast.
40	Mar. 25–26	65 –56	22–9 E.	SW.	33.6	725	Northeast.
41	Apr. 24–25	50 –55	6 E.–3 W.	NW.	22.0	748	Coalesced.

and 41 are of this class. It will be noticed that in three cases the same number occurs in three of the preceding lists, and in twenty cases the same number occurs in two of these lists.

61. It may be objected that if I assign two or three causes for the same phenomenon the probability is that I have failed to discover the true cause. To this objection it may be answered, that the three causes here assigned are all intimately related to each other, and may all concur in the same phenomenon, or in different stages of the same phenomenon. It will be shown hereafter that the surface winds blow outward from an area of high pressure, and circulate around the center from left to right, as shown in the outer portions of Plate XIV, Fig. 2. Now suppose that two areas of high pressure are situated at a distance from each other not much greater than the sum of their radii, and that the air between them is quiet, and the pressure does not differ much from 760mm or 30 inches. These two areas of high pressure exert an influence to set the air between them in motion, in such a manner as to circulate from right to left about a center. The inward motion which attends this circulation cannot exist unless a portion of the air within this area ascends above the earth's surface. This air, in ascending, becomes cooled, a portion of its vapor is

condensed, and is precipitated in the form of rain or snow. The heat, liberated in the condensation of this vapor, causes a stronger upward movement of the air, the inward movement of the air is accelerated, and the barometer falls for a reason to be explained hereafter. Thus may be formed an area of low pressure as exhibited in Plate XIV, Fig. 2, within which the winds circulate with great velocity, and there is an abundant precipitation of rain or snow. If there is a second area of low pressure within a moderate distance these two systems of circulating winds may unite to form a single system of winds, and thus the two low centers may coalesce. Whenever the first case exists (viz, two areas of high pressure not very remote from each other), there generally results a fall of rain, with the development of a new low center, and this low center under favorable circumstances will coalesce with another low center in its vicinity.

62. There remain five cases not included in either of the preceding lists, viz, Nos. 25, 32, 33, 38, and 39, and each of these cases bears some resemblance to Class IV for the United States, Art. 58. In No. 25 the pressure was below 30 inches throughout the whole of Asia and a large part of Europe, and there were several centers about which there prevailed feeble systems of circulating winds. On the western side of the low, near the Ural Mountains, the gradients were feeble and the pressure on that side was below 30 inches for a distance of 2,000 miles. Under such circumstances a small force was sufficient to change the position of the point of least pressure. If the isobars for this region could be drawn in a reliable manner for each tenth of an inch they would probably indicate more definitely the nature of this force.

No. 32 was similar to the preceding. In no part of the northern hemisphere did the pressure rise much above 30 inches, and in nearly the whole of Europe and Asia the pressure was below 30 inches.

In No. 33 the pressure was also below 30 inches in nearly every part of Europe and Asia. These three cases were quite similar, and the abnormal movement of the low center was probably due to the same cause.

In No. 38 the gradients on the eastern side were considerable, but on the western side they were slight, and the low center appeared to be crowded westward by an area of high pressure (30.4 to 30.6), which followed it persistently on the northeast side, while the pressure on the southwest side for a great distance was below 30 inches.

No. 39 was similar to the preceding after the morning of the 21st, and in each of the last two cases an area of high pressure seemed to exert a decided influence in crowding the low center westward. Each of these five cases was attended by some rainfall, but the amount reported in the published observations does not seem to present an adequate cause for the abnormal movement of the low centers.

63. The preceding discussion seems to warrant the following conclusions, viz: That the westerly movement of low centers, which is occasionally observed in the middle latitudes of Europe and America, is generally due to one or more of the following causes:

1. The influence of one low area upon an adjacent low area, which influence sometimes seems to act as an attractive force.

2. The influence of a considerable fall of rain or snow, which also acts as an attractive force.

3. The influence exerted by two areas of high pressure, not very remote from each other, by which means a new movement is imparted to the air included between them, and a new low center is sometime developed.

4. The influence of an area of high (or only moderately high) pressure, on the northeast side of a low area, when the gradients on the southwest side of the low area are slight, in which case the center of the low area may be crowded towards the southwest.

If these causes are sometimes sufficiently powerful to divert the center of a low area westward, it may be presumed that there are many more cases in which these causes are sufficiently powerful to affect, in an appreciable degree, both the direction and velocity of the movement of a low center.

64. The facts which have been stated in the preceding pages seem to afford a basis for some general conclusions respecting the movement of storm areas. Many meteorologists have claimed that the progressive movement of storm areas is satisfactorily explained by saying that they are carried forward by the general movement of the mass of the atmosphere within which they are

formed; that is, they *drift*, in a sense similar to that in which waves, eddies, &c., formed on the surface of a river, drift with the current. They advance as the water of the river advances, and in the same direction. But we have found that the average direction of movement of areas of low barometer does not generally correspond with the average direction of the wind for the same region. This is seen not only in the case of tropical storms but also in storms of the middle latitudes. Near the West India Islands the average direction of storm tracks, while the storms are moving westward, differs about 30 degrees from the average direction of the wind for the same season of the year. In the China Sea the average direction of storm tracks is nearly at right angles with the average direction of the wind, and the average direction is nearly the same during those months in which the prevalent wind is from the southwest as during those months in which the prevalent wind is from the northeast. In the western part of the Atlantic Ocean, near latitude 50°, the average direction of storm paths is about 30° more northerly than that of the average wind, and in the eastern part of the Atlantic Ocean, near latitude 55°, it is almost 30° more southerly. In the northwestern part of the United States, between the Rocky Mountains and the meridian of 90° from Greenwich, we find places where the average direction of storm tracks is 45° more northerly than that of the wind, and other places where it is 20° more southerly than that of the wind.

65. But it may be claimed that the progress of storm areas is not determined entirely by the average movement of the atmosphere, but by that movement which is taking place at the date of the storm. I have endeavored in the preceding pages to investigate this question, and to present the evidence for the above hypothesis in the most favorable light, but if we scan the evidence critically we must conclude that it is entirely unsatisfactory. If we claim that the progressive movement of a storm area is due to the progressive movement of the general mass of the atmosphere in which it is formed it seems necessary to admit that a mass of the atmosphere of considerably greater extent than the storm area is advancing in the same direction and at the same rate as the storm advances. In order to decide whether such is the fact we need only consult a well-constructed weather map of sufficient dimensions to include not merely a storm area but a considerable margin beyond it. The storm represented on Plate III had an average diameter of 2,500 miles, and during the twenty-four hours succeeding the date of the map it advanced about 350 miles towards the northeast. If the movement of this storm area was due to a general drift of the atmosphere then this drift must have included not merely the area within the isobar 30 inches, but also the adjacent areas of high pressure which clung persistently to the low area. This map seems to be too limited to furnish the required information in a form which is entirely satisfactory, and it is desirable to have similar maps for several successive days. The Signal Service maps afford abundant materials for the proposed purpose, and Hoffmeyer's maps are still better, since they include a much larger portion of the earth's surface. If we open Hoffmeyer's Atlas anywhere at random we shall not find the mass of the atmosphere in the rear of a storm moving forward in the same direction as that in which the storm advances. Plates VIII and IX accompanying this pamphlet are thought to be decisive on this point. The storm maps of the United States furnish similar testimony. Plates I and II show that the general movement of the atmosphere in the rear of a storm is not in the same direction as that in which the storm center advances, and the evidence would be still clearer if the maps included a larger area. A slight examination of the United States weather maps, or of Hoffmeyer's charts, must satisfy any one that the general mass of the atmosphere surrounding a great storm is not advancing in the same direction as that in which the storm center advances.

66. If we follow the progressive movement of a great storm from day to day by means of maps representing the phenomena at intervals not greater than eight hours we shall find that in front of the storm the air appears to be drawn in towards the center, by which means the pressure on the front side of the storm is diminished. The air thus drawn in towards the center rises to a considerable elevation above the surface of the earth, and its vapor is condensed. In the rear of the storm the exterior air rushes in and restores the pressure on that side, and as the result of this double process the point of least barometric pressure is carried forward. This movement of the exterior air in the rear of a storm is not necessarily in the same direction as that in which the

storm center advances. In the United States storms almost invariably advance eastward, and generally towards a point a little north of east; but the wind which presses upon the rear generally comes from the north or northwest, which direction is often at right angles, or nearly at right angles, with the direction in which the storm center advances. Plates I and II exhibit this fact, and the same is substantially true of nearly every great storm shown on the Signal Service maps. This movement of the air by which the center of least pressure is carried forward bears some analogy to the movements which cause the advance of a wave upon the surface of the ocean, and hence we may with propriety say that the progressive movement of a storm area is the movement of a great atmospheric wave.

67. Besides the general considerations here stated there are various special phenomena which indicate that the movement of areas of low pressure cannot be fully explained by the theory of a general drift of the atmosphere. We frequently find two neighboring low areas advancing in directions inclined to each other at an angle of 45°, or even a greater angle. In the United States, while a low center is advancing from Florida along the Atlantic coast, towards the northeast, another low center may be advancing eastward over the region of the Great Lakes, and the two low centers may coalesce somewhere in the neighborhood of Nova Scotia or Newfoundland. It will be seen from Plate X that the storms which proceed from the Gulf of Mexico, and from the neighborhood of the West India Islands, generally advance towards Newfoundland, and the storms which come from the northwestern part of the United States also tend towards the same region. Newfoundland becomes thus a point of convergence of storm tracks, proceeding from regions quite remote from each other. In the vicinity of Newfoundland there exists some influence which appears to act as an attractive force upon storm centers. This influence probably results from the great amount of precipitation near that island, arising from the proximity of the warm water of the Gulf Stream to the colder air from the land. Plate XI shows other points towards which storm tracks seem to converge, particularly the Asiatic coast near Japan, and this fact probably results from a cause similar to the one just named. If Plates X and XI exhibited the storm tracks of different regions according to the relative frequency of their occurrence, other points of convergence of storm tracks would be exhibited. Along these converging storm paths two storms often travel simultaneously and coalesce in a single storm area. Such a movement appears inconsistent with the drift theory.

68. For the convenience of those persons who may wish to investigate cases of this kind for themselves I present the following list, which shows some of the most decided cases in which two centers of low pressure in the United States have coalesced. They are taken from the Signal Service weather maps for the years 1873–1880. These maps show a considerable number of other cases of like kind, some of which have been omitted because the depression of the barometer was small, and others because the position of the low center was not very sharply defined, or was situated near the margin of the weather map:

Examples in which two centers of low pressure approach each other and coalesce.

1873,	Mar. 29. 1–29. 2	1874,	Sept. 25. 1–25. 3	1878,	Mar. 13. 3–14. 1	1879,	Oct. 16. 1–17. 1
	Oct. 4. 3– 5. 1	1875,	Jan. 22. 2–22. 3		May 2. 1– 2. 2		Oct. 28. 2–28. 3
	Oct. 11. 1–11. 3		Nov. 10. 1–10. 2		June 18. 1–18. 2		Nov. 20. 1–20. 2
1874,	Apr. 19. 2–19. 3	1876,	Mar. 25. 3–26. 1		Nov. 22. 1–22. 2	1880,	Feb. 13. 1–13. 2
	Apr. 25. 2–25. 3	1877,	Dec. 29. 3–30. 1	1879,	Jan. 1. 3– 2. 2		Mar. 7. 2– 8. 1
	Aug. 30. 2–31. 1	1878,	Feb. 14. 3–15. 1		Feb. 4. 1– 4. 2		Oct. 29. 2–29. 3

Among these twenty-four cases there are only three in which the paths of the two low centers were not inclined to each other at an angle as great as 45°; in half of the cases the two paths were inclined at an angle considerably greater than 45°; in eight or nine of the cases the angle was nearly as great as 90°; and in three of the cases the angle was greater than 90°.

69. It sometimes happens that within an area of low pressure, having but a single center, a second low center is developed. The following list shows twenty-four such cases selected from the Signal Service maps for 1873–1880. The maps show a large number of other similar cases; but in

the cases here cited the depression of the barometer was generally considerable, and the position of the low centers was distinctly indicated:

Cases in which a second low center is developed within an area of low pressure.

1873,	Feb. 18.1–18.2	1874,	Nov. 23.1–23.3	1876,	Mar. 25.2–25.3	1878,	Mar. 12.3–13.1
	Feb. 20.1–20.2	1875.	Jan. 24.2–24.3		May 7.1– 7.2		Nov. 23.3–24.1
	Mar. 28.2–29.1		Jan. 30.3–31.2		May 7.2– 7.3	1879,	Mar. 29.1–29.2
1874,	Apr. 25.1–25.2		May 1.2– 1.3	1877,	Dec. 29.2–29.3	1880,	Jan. 21.3–22.1
	Aug. 29.1–29.2		Nov. 3.1– 3.2	1878,	Jan. 13.2–13.3		Feb. 12.2–12.3
	Aug. 30.1–30.2	1876,	Mar. 5.3– 6.1		Jan. 30.2–31.1		Apr. 17.1–17.2

In a majority of these cases the two low centers appear to have subsequently coalesced; but in several of them the two low centers moved off in directions inclined to each other at an angle of 90° or more, and with unequal velocities.

70. Over the Atlantic Ocean and Europe cases similar to the preceding are of much more frequent occurrence than in the United States; the depression of the barometer is generally much greater, and the low areas have a much greater geographical extent. By consulting Hoffmeyer's weather maps we may easily find examples in which two low centers move towards each other from nearly opposite directions and coalesce; and we may also find frequent cases in which a great area of low pressure, with but one center, undergoes a change by which two low centers are developed, and these new low centers recede from each other. Sometimes there is a further change by which three or four, or even more, low centers are formed, and these low centers have a progressive movement in different directions, and with unequal velocities. On the contrary, within a large area of low pressure showing several low centers, a low center may disappear from simple changes of pressure. In like manner a second low center may disappear, and so on. Plate IX shows five low centers within a single area of low pressure. The map for the preceding day showed only three low centers within the same low area, while the map for the following day showed five low centers, but one of them had no connection with either of the five shown on Plate IX. In cases like this the changes in the position and magnitude of the low centers are so rapid that, in comparing two weather maps for successive days, we frequently find it impossible to identify a low center on one of the maps, with its corresponding low center on the other map.

71. Examples may be easily found to illustrate all of these different cases; but for the convenience of those who may wish to examine such cases without the trouble of searching for them the following lists are given, and the cases are all taken from Hoffmeyer's charts for 1875:

1. *Examples in which two centers of low pressure approach each other and coalesce.*

Jan.	3– 4	Mar.	23–24	May	11–12	Sept.	24–25	Oct.	29–30	Nov.	30–31
	8– 9	Apr.	3– 4		21–22		27–28	Nov.	1– 2	Dec.	4– 5
	11–12		11–12	June	14–15	Oct.	8– 9		3– 4		6– 7
Feb.	25–26		20–21	July	2– 3		9–10		5– 6		13–14
Mar.	3– 4		25–26	Aug.	15–16		11–12		6– 7		26–27
	10–11	May	2– 3		28–29		13–14		15–16		27–28
	12–13		6– 7	Sept.	18–19		21–22		27–28		

2. *Examples in which, within an area of low pressure, two or three low centers are developed where only one had existed previously.*

Jan.	2– 3	Sept.	8– 9	Nov.	4– 5	Nov.	13–14	Dec.	8– 9
Apr.	6– 7		23–24		8– 9		21–22		21–22
June	21–22	Oct.	14–15		11–12		26–27		28–30
	24–25	Nov.	2– 3		12–13	Dec.	5– 6		

In several of these cases the two low centers appear to have started from the same low center and thence receded from each other. This is particularly true for January 2–3; April 6–7; June 24–25; September 8–9; November 11–12; November 13–14; December 8–9; and December 21–22.

3. *Examples in which, within an area of low pressure, a new low center is developed by changes of pressure occurring within the low area.*

Jan.	18–19	Apr.	2– 3	June	5– 6	Aug.	25–26	Oct.	10–11
Mar.	2– 3		4– 5		16–17		26–27		25–26
	4– 5		12–13	July	13–14	Sept.	20–21	Nov.	7– 8
	6– 7	May	3– 4		21–22		25–26		9–10
	27–28		20–21	Aug.	13–14	Oct.	6– 7	Dec.	22–23

4. *Examples in which, within an area of low pressure showing several low centers, one or more of them disappears by changes of pressure.*

Jan.	5– 6	Feb.	9–10	Mar.	1– 2	July	14–15	Nov.	11–12
	19–20		10–11		13–14		22–23		
	22–23		15–16	Apr.	5– 6	Nov.	10–11		

It surely will not be claimed that in these cases the movement of the low centers can be ascribed to a simple drifting of the general mass of the atmosphere in which the low areas were formed.

72. If we reject the drift theory it will doubtless be asked how can we explain the fact that in the middle latitudes storms almost invariably advance toward the east, and the opposite movement only occurs occasionally, and seldom continues longer than one or two days. This fact seems to result from the prevalent movement of the wind toward the east, but the result is due, not to a general drifting of the mass of the atmosphere within which the low area is formed, but to the fact that the pressure on the west side of the low area is more steady and persistent than that on the east side. The characteristic features of a great storm movement are a motion of the air from all sides spirally inward, together with an upward movement, resulting in the condensation of vapor at various places within the low area. Now if the air pressed in with equal force on all sides of the low center, and if there was an equal precipitation of vapor on all sides, no reason is apparent why the low center should advance at all. It sometimes happens that the pressure on the west side is very small, while there is considerable pressure on the east side, and in such cases the low center moves towards the west. Examples of this kind have been given in article 58, viz, Nos. 2, 3, 7, 15, 18, 27, 28, and 29 of Table XXII; and in article 62, viz, Nos. 25, 32, 33, 38, and 39 of Table XXVIII. But this movement towards the west cannot be long maintained. In the middle latitudes the east winds are exceptional and result mainly from disturbances caused by storms. On the contrary the west winds result from general causes, which are permanent in their character and are independent of storms; and if there were no storms the west winds would rarely be interrupted. During the prevalence of an east wind the causes which produce west winds are not destroyed, their influence is only temporarily suspended, and they soon return with a force not impaired but rather augmented by their temporary suspension. The pressure on the west side of

TABLE XXIX.—*Rate of progress of storm centers in the United States.*

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1872....	31.2	29.4	34.5	34.5	24.7	21.8	24.6	18.3	22.2	20.9	23.6	28.8
1873....	25.8	32.7	28.1	22.3	23.5	20.8	24.6	17.8	23.1	28.1	27.9	26.7
1874....	23.0	33.9	29.8	31.4	22.2	22.4	25.9	19.9	23.1	28.5	30.3	32.7
1875....	32.1	32.8	30.0	26.4	29.2	31.5	25.3	17.1	30.5	23.4	30.0	31.1
1876....	38.1	31.5	26.4	23.6	24.7	19.3	26.4	23.2	23.8	27.7	22.6	38.3
1877....	37.7	26.5	32.6	25.2	27.3	25.2	24.2	20.0	17.4	20.2	25.5	24.7
1878....	26.3	27.7	24.3	22.6	17.9	18.4	21.7	26.8	23.9	19.6	21.2	34.0
1879....	35.5	33.3	35.1	27.8	25.3	29.4	26.4	21.0	21.7	30.8	40.7	38.7
1880....	37.6	39.6	35.8	27.2	25.1	24.5	25.7	25.9	23.5	22.3	34.1	38.8
1881....	32.3	35.4	26.8	37.1	32.6	32.8	26.6	25.4	30.6	37.5	30.8	33.6
1882....	42.1	41.6	34.8	29.5	21.6	26.8	19.8	19.9	23.5	27.7	27.7	30.2
1883....	39.8	36.4	38.0	28.1	30.0	24.2	25.8	28.0	25.0	37.3	39.4	33.0
1884....	38.6	43.9	33.3	21.5	26.8	20.5	22.1	30.7	32.6	34.4	35.2	43.7
Mean.	33.8	34.2	31.5	27.5	25.5	24.4	24.6	22.6	24.7	27.6	29.9	33.4

storm areas is thus a strong and persistent one, while that on the east side results from temporary causes and cannot be long maintained. It occasionally happens during a violent storm that the east winds are stronger than the west winds. In such a case the low center may be pushed westward, but such a result does not necessarily follow, for a large part of the air which pushes in on

TABLE XXX.—*Barometric minima advancing at least 1,000 miles in twenty-four hours.*

No.	Date.	First station.	Barometer.	Second station.	Barometer.	Progress.	Change of barometer in twenty-four hours. First station.	Change of barometer in twenty-four hours. Second station.	Rain in low. East.	Rain in low. West.	Wind in low. East.	Wind in low. West.	High following.
			Inch.		*Inch.*	*Miles.*							
	1872.												
1	Nov. 6.2	Pembina.....	29.45	Portland	29.12	1,376	+0.36	—1.06	0.15	0.02	9.49	12.39	30.09
2	24.2	Keokuk	.62	Quebec........	.39	1,214	+ .55	— .68	.02	.01	8.31	12.81	.33
3	Dec. 14.1	Omaha	.67	Montreal	.51	1,132	+ .71	— .29	.02	.00	12.27	16.97	.46
4	19.1	Indianola ...	.70	Buffalo........	.43	1,295	+ .32	—1.06	.36	.20	9.89	10.33	.37
	1873.												
5	Jan. 4.3	Memphis	.74	Portland	.33	1,215	+ .54	— .91	.38	.13	9.16	13.52	.30
6	26.3	Lake City ...	.80	Halifax	.17	1,404	+ .20	—1.04	.19	.05	7.33	9.22	.92
7	Feb. 15.3	Memphis	.59	New London ..	.53	1,055	+ .49	— .95	.27	.05	7.73	8.83	.11
8	May 12.2	Saint Paul...	.44	Portland	.35	1,145	+ .45	— .24	.04	.01	10.09	10.91	.02
9	Nov. 3.3	Duluth	.49	Father Point. .	.60	1,100	+ .86	— .31	.00	.00	12.28	12.30	.42
10	23.1	Indianola....	.73	Pittsburgh	.48	1,270	+ .39	— .80	.24	.14	7.81	11.20	.12
11	24.1	Pittsburgh ..	.48	Halifax	28.82	1,002	— .09	—1.40	.38	.13	14.15	10.98	.29
	1874.												
12	Jan. 3.3	Escanaba ...	.31	Father Point ..	29.55	1,000	+1.07	— .38	.03	.06	16.05	17.61	.57
13	Feb. 6.2	Louisville ...	.82	Sydney	.70	1,212	+ .39	+ .01	.16	.10	7.90	7.46	.22
14	19.1	Duluth	.52	Father Point..	.70	1,123	+ .46	— .58	.06	.01	7.96	10.66	.17
15	22.1	Indianola....	.89	Rochester	.58	1,374	+ .55	— .66	.24	.21	7.47	15.04	.72
16	23.1	Rochester....	.58	Father Point ..	.47	1,058	+ .95	— .21	.21	.07	8.34	21.90	.82
17	Mar. 3.1	Leavenworth.	.29	Ottawa	.34	1,114	+ .74	— .49	.16	.14	10.40	16.66	.30
18	18.2	Keokuk......	.63	Quebec........	.19	1,187	+ .37	— .73	.09	.05	5.80	11.36	.13
19	Apr. 5.1	Saint Louis..	.71	Ottawa	.63	1,065	+ .38	— .55	.12	.03	9.84	11.65	.17
20	Sept. 2.1	Saint Paul...	.89	Father Point..	.72	1,175	+ .41	— .24	.03	.01	4.21	9.00	.42
21	Nov. 28.1	Mobile.......	30.15	Quebec	.54	1,466	+ .19	— .81	.17	.14	15.66	15.33	.62
22	Dec. 2.2	Marquette .	29.44	Cape Rozier...	.64	1,095	+ .91	— .40	.03	.00	14.33	14.58	.35
23	13.2	Cleveland ...	.83	Halifax.......	.42	1,092	+ .54	— .85	.11	.04	9.00	11.27	.57
24	16.1	Omaha	.51	Ottawa	.59	1,065	+1.08	— .69	.06	.03	11.39	15.08	.73
25	23.2	Marquette ...	.49	Halifax.......	.37	1,165	+ .61	— .37	.03	.02	10.79	15.80	.42
26	27.2	Bismarck	.59	Quebec	.59	1,134	+ .79	— .56	.11	.09	9.26	7.88	.55
	1875.												
27	Jan. 1.3	Marquette ...	.91	Halifax.......	28.89	1,170	+ .11	—1.11	.23	.16	6.23	17.78	.54
	1877.												
28	Jan. 1.1	Saint Marks..	.62	Boston........	29.35	1,126	+ .67	— .67	.28	.06	13.92	14.08	.73
29	6.1	Mobile	.84	New York.....	.15	1,048	+ .23	—1.18	.31	.08	13.63	11.62	.42
30	7.3	Indianola....	.84	Eastport	.37	1,872	+ .56	— .01	.04	.02	8.47	16.49	.40
31	15.1	Fort Gibson..	.45	Malone	.56	1,270	+ .89	— .56	.23	.07	10.42	17.53	.46
32	19.1	Bismarck	.63	Parry Sound..	.19	1,080	+ .23	—1.03	.05	.01	9.05	15.85	.30
33	Mar. 1.2	Memphis	.56	Parry Sound..	.07	1,003	+ .30	— .98	.24	.05	12.00	13.79	.04
34	3.2	Indianapolis .	.61	Father Point..	.52	1,178	+ .66	— .15	.08	.02	11.19	13.77	.36
35	6.2	Escanaba	.59	Chatham	.71	1,055	+ .61	— .26	.09	.02	14.87	16.59	.37
36	8.2	Cincinnati...	.16	Father Point..	28.74	1,061	+ .91	1.38	.28	.06	20.10	18.26	.43
37	15.3	Dodge City.	.56	Knoxville	29.64	1,050	+ .84	— .50	.08	.03	11.46	19.93	.51
38	18.3	Leavenworth.	.66	Cape Henry...	.87	1,209	+ .27	— .34	.06	.00	11.65	13.92	.19
39	20.2	Saint Louis..	.55	Malone	.57	1,047	+ .50	— .52	.13	.03	10.48	12.70	.11
40	Oct. 28.1	Leavenworth.	.56	Rockliffe......	.57	1,077	+ .67	— .54	.05	.03	11.30	12.10	.41
41	Nov. 1.1	San Antonio.	.58	Erie	.11	1,122	+ .52	— .91	.25	.11	9.48	9.87	.25
42	5.1	Indianapolis.	.84	Halifax	.50	1,151	+ .67	— .83	.22	.05	14.25	14.44	.56
43	8.2	Toledo.....	.47	Chatham	.57	1,128	+ .69	— .54	.43	.11	17.22	13.09	.54
		Means.......	29.62	....	29.42	1,167	+ .55	— .65	0.16	0.06	10.76	13.78	30.39

the east side rises from the earth's surface, while the air which pushes in on the west side does not rise at all, or not to an equal extent. Thus the low area is filled up on the west side, and were it not for the continued precipitation of vapor the low area would soon become obliterated. In the subsequent pages additional facts will be presented, showing the unsatisfactory nature of the drift theory.

Rate of progress of areas of low pressure.

73. In order to exhibit the average velocity with which centers of low pressure advance over the United States, I have prepared Table XXIX, which shows, in miles per hour, the average velocity of storm centers for each month during a period of thirteen years, according to the observations of the United States Signal Service.

We see from this table that the average velocity of progress of storms for the entire year is 28.4 miles; also that the velocity is greatest in February and least in August, and that the former velocity is 50 per cent. greater than the latter. We also see that the velocity varies very much for the same month in different years, the greatest mean velocity for several of the months being nearly double the least mean velocity for the same months.

74. In order to discover, if possible, the cause of these unequal movements, I have selected the most remarkable cases of extremely rapid motion, and also the cases of extremely slow motion, during the period for which the tri-daily observations of the Signal Service have been published, viz, from September, 1872, to January, 1875, inclusive, also for the year 1877, making in all forty-one months of observations. Table XXX shows the cases in which a storm center has advanced at least 1,000 miles in twenty-four hours.

Column 1 gives the number of reference; column 2 the date at which the rapid motion commenced, where the figures 1, 2, and 3, attached to the day of the month, denote the first, second, and third of the hours of observation for the given day; column 3 shows the station at which the barometer, at the given date, was lowest; column 4 the height of the barometer at the station named; column 5 shows the station at which the barometer was lowest twenty-four hours after the date given in column 2; column 6 the height of the barometer at the station named in the preceding column; column 7 shows in miles the movement of the low center during twenty-four hours, as indicated by the isobars, which best represent the Signal Service observations; column 8 the change in the barometer at the stations named in column 3, during the day here considered; (+ denotes increasing pressure, − denotes decreasing pressure); column 9 the change in the barometer at the stations named in column 5 during the same day; column 10 shows the average rain-fall at all the stations within the low area (determined by the isobar 30 inches), on the east side of the low center, for each period of eight hours during the given day. These numbers should therefore be multiplied by three, in order to show the average rain-fall for the day in question; column 11 shows the average rain-fall at all the stations within the low area on the west side of the low center, for each period of eight hours; column 12 shows the average velocity of the wind (in miles per hour), at the stations within the low area on the east side of the low center; and column 13 shows the average velocity of the wind at the stations within the low area on the west side of the low center. Generally the retreat of the low area eastward was immediately succeeded by an area of high pressure on its western side. Column 14 shows the highest pressure observed at any station within this area of high pressure; and when there was no succeeding area of high pressure, it shows the highest pressure immediately succeeding the area of low pressure. At the bottom of the table are given the means of the numbers in ten of the columns.

75. Table XXXI shows the cases in which a storm center has advanced not more than 240 miles in twenty-four hours. The arrangement of this table is similar to that of Table XXX. The Signal Service observations show a considerable number of other cases, which, perhaps, ought to be included in these tables, but which are omitted on account of the uncertainty respecting the exact position of the center of low pressure.

76. The following are some of the results derived from a comparison of these two tables:

1. For the cases in Table XXXI the average pressure at the low center was the same at the close of the given day as at its beginning; that is, the storms neither increased nor diminished in intensity. For the cases in Table XXX the average pressure at the low center was 0.20 inch less at the close of the given day than at its beginning; that is, the storms increased considerably in intensity during the day in question.

2. The average rate of progress of the storms named in Table XXX was more than seven times as great as that of the storms named in Table XXXI.

3. In the cases named in Table XXXI the barometer fell, on an average, 0.09 inch in twenty-four hours in front of the storm, and rose 0.09 inch in the rear of the storm. In the cases named in

Table XXX the barometer fell, on an average, 0.65 inch in twenty-four hours in front of the storm, and rose 0.55 inch in the rear of the storm; that is, during the days named in Table XXX the oscillation of the barometer was nearly seven times as great as during the days named in Table XXXI.

TABLE XXXI.—*Barometric minima advancing not more than 240 miles in twenty-four hours.*

No.	Date.	First station.	Barometer.	Second station.	Barometer.	Progress.	Change in barometer in twenty-four hours: First station.	Change in barometer in twenty-four hours: Second station.	Rain in low: East.	Rain in low: West.	Wind in low: East.	Wind in low: West.
			Inch.		*Inch.*	*Miles.*						
	1872.											
1	Sept. 5.1	Omaha......	29.51	Omaha.......	29.51	156	.00	.00	0.04	0.02	6.11	7.52
2	24.2	Duluth......	.31	Duluth.......	.37	72	+.06	+.06	.10	.06	15.33	17.78
3	25.1	Duluth......	.33	Marquette ...	.43	208	+.16	+.05	.08	.01	14.65	14.95
	1873.											
4	Apr. 12.1	Cape May...	.65	Boston	.61	238	+.14	−.31	.20	.07	15.77	15.14
5	14.1	Omaha......	.46	Leavenworth	.47	97	+.12	−.03	.05	.05	11.40	25.43
6	16.2	Cleveland ...	.71	Chicago......	.58	218	−.04	−.25	.06	.04	10.13	10.70
7	May 21.3	Fort Sully...	.46	Breckenridge.	.35	215	+.25	−.26	.04	.04	6.80	12.86
8	June 21.2	Fort Sully...	.46	Fort Sully....	.30	40	−.16	−.16	.07	.02	8.23	8.55
9	July 15.2	Fort Sully...	.48	Breckenridge.	.47	240	+.12	−.17	.13	.06	9.76	11.46
10	21.2	Fort Sully...	.30	Fort Sully ...	.68	88	+.38	+.38	0	0	12.79	15.11
11	Aug. 13.2	Fort Sully...	.58	Yankton	.66	181	+.12	−.17	.08	.04	8.41	10.14
12	Sept. 2.2	Fort Sully...	.38	Omaha.......	.44	240	+.15	−.35	.08	.03	7.92	11.20
13	10.1	Fort Garry ..	.62	Pembina	.61	189	.00	−.19	0	0	6.73	9.75
	1874.											
14	Mar. 9.1	Eastport	.24	Eastport	.03	129	−.21	−.21	.14	.02	5.45	20.45
15	16.1	Fort Sully...	.26	Fort Garry ...	.35	240	+.27	−.35	.08	.04	12.08	11.67
16	Apr. 30.1	Father Point	.11	Father Point .	.05	90	−.06	−.06	.07	.02	10.91	16.90
17	May 1.1	Father Point	.05	Sydney.......	.48	184	+.51	−.06	.06	.02	7.58	11.91
18	9.2	Fort Sully...	.01	Breckenridge.	.29	217	+.37	−.10	.12	.00	14.45	14.64
19	22.3	Fort Sully...	.57	Fort Sully....	.31	0	−.26	−.26	.02	.01	6.50	10.50
20	26.2	Fort Sully...	.16	Fort Sully....	.16	0	.00	.00	.01	.00	10.33	12.00
21	27.2	Fort Sully...	.16	Fort Sully....	.27	0	+.11	+.11	.02	.01	10.25	14.80
22	28.2	Fort Sully...	.27	Breckenridge.	.77	218	+.64	+.23	.03	.00	10.55	11.10
23	June 18.1	Portland	.50	Boston	.85	125	+.38	+.22	.25	.02	11.87	16.35
24	26.2	Fort Sully...	.33	Yankton	.27	180	−.04	−.38	.07	.01	8.67	8.60
25	July 2.2	Fort Sully...	.45	Fort Sully....	.55	0	+.10	+.10	.04	.00	8.82	13.13
26	17.3	Fort Sully...	.65	Fort Garry ...	.59	217	+.06	−.12	.00	.00	8.34	9.67
27	Aug. 5.2	Leavenworth	.63	Leavenworth.	.73	151	+.10	+.10	.03	.02	5.26	7.50
28	9.3	Fort Sully...	.54	Yankton	.37	179	−.04	−.31	.04	.01	5.25	9.87
29	27.1	Omaha......	.61	Omaha.......	.54	111	−.07	−.07	.12	.01	9.00	10.80
	1877.											
30	Feb. 13.1	Saint John ..	.41	Port Hastings	.59	211	+.63	+.05	.14	.09	14.37	20.61
31	17.2	Chatham....	.16	Chatham.....	.22	73	+.06	+.06	.30	.10	15.67	18.12
32	Mar. 27.2	Barnegat....	.22	Boston	.09	237	+.21	−.24	.13	.12	11.14	18.05
33	30.2	Fort Sully...	.17	Yankton	.14	164	−.28	−.52	.11	.06	11.63	17.14
34	Apr. 5.3	Eastport	.45	Eastport	.49	150	+.04	+.04	.27	.02	13.93	13.48
35	16.2	North Platte	.18	Omaha.......	.28	210	+.10	−.25	15	.11	9.54	14.50
36	17.3	Omaha......	.44	Leavenworth.	.11	159	−.06	−.36	.08	.01	10.27	20.24
37	May 18.3	Fort Sully...	.55	Bismarck	.30	126	−.11	−.33	.11	.01	11.84	13.75
38	20.2	La Crosse ...	.36	Saint Paul...	.45	240	+.15	+.06	.14	.06	8.18	14.31
39	29.2	Bismarck....	.10	Bismarck	28.79	0	−.31	−.31	.00	.01	14.75	15.75
40	30.2	Bismarck....	28.79	Bismarck	.97	80	+.18	+.18	.18	.04	12.90	13.33
41	June 22.2	Bismarck....	29.29	Bismarck	29.17	0	−.12	−.12	.12	.03	10.88	14.66
42	July 26.2	Fort Sully...	.24	Bismarck	.49	240	+.41	+.18	.15	.07	8.11	10.36
43	29.3	Bismarck....	.43	Bismarck	.48	180	+.05	+.05	.02	.04	11.37	9.83
44	Aug. 4.2	Bismarck....	.41	Fort Sully....	.55	184	+.22	+.01	.01	.02	8.35	9.14
45	Sept. 10.2	Bismarck....	.33	Bismarck	.30	183	−.03	−.03	.03	.00	7.93	9.71
46	13.1	Bismarck....	.34	Bismarck	.13	187	+.09	+.09	.02	.04	9.39	14.31
47	19.2	Mobile	.49	Saint Marks..	.67	168	+.25	0	.51	.07	15.54	11.52
48	20.3	Bismarck...	.53	Fort Sully....	.35	232	−.11	−.28	.00	.00	11.06	13.60
49	28.2	Bismarck....	.38	Fort Sully....	.29	210	−.01	−.11	.01	.00	9.83	12.27
50	Oct. 2.2	Mobile	.52	Saint Marks..	.43	240	+.11	−.13	.30	.03	14.02	16.82
51	24.2	Bismarck....	.52	Bismarck	.31	192	−.21	−.21	.07	.00	9.00	8.66
52	25.2	Bismarck....	.31	Bismarck	.50	140	+.19	+.19	.04	.01	6.57	8.26
		Mean	29.38	....	29.39	156	+.09	−.09	.09	.03	10.30	13.24

4. During the days named in Table XXXI the average daily rain-fall for the entire low area was 0.27 inch on the east side of the low center and 0.09 inch on the west side. During the days named in Table XXX the average daily rain-fall for the entire low area was 0.48 inch on the east side of the low center and 0.18 inch on the west side. In each case the rain-fall on the east side of the low center was about three times as great as on the west side, and for the storms in Table XXX the average rain-fall was about double that for the storms in Table XXXI.

5. For the storms in Table XXX the average velocity of the wind was 3.02 miles per hour greater on the west side of the low center than it was on the east side. For the storms in Table XXXI the average velocity of the wind was 2.94 miles per hour greater on the west than on the east side. For the storms in Table XXX the average velocity of the wind was a half mile per hour greater than for the storms in Table XXXI.

77. We conclude from these results that the velocity of the wind, within an area of low pressure, has very little influence upon the rate of progress of the low center. Moreover, the rain-fall within the low area cannot be the sole cause, and probably is not the principal cause, of the very rapid progress which the center of low pressure sometimes exhibits, for in sixteen of the cases in Table XXX the rain-fall was less than the average rainfall for the cases in Table XXXI. In No. 9 of Table XXX no rain fall was reported at any station within the low area either on the east or west side during the twenty-four hours named, and in Nos. 2, 3, 12, 22, 24, and 30 the amount of rain-fall was quite insignificant. On the other hand, in seven cases of Table XXXI the rain-fall was greater than the average rain-fall in Table XXX, and in No. 47 the rain-fall on the east side of the low center was greater than in any storm named in Table XXX.

The facts which the two tables exhibit most strikingly in contrast are that in the cases of rapid progress the storms generally were increasing in intensity, and the extent of the oscillation of the barometer in twenty-four hours was almost exactly proportional to the rate of progress of the storm center.

78. In order to discover the causes which were most influential in accelerating the movements in Table XXX, and retarding the movements in Table XXXI, I have carefully examined each case, and have found the following results:

In twenty of the cases in Table XXX the movement of the low center appeared to be accelerated by the influence of a low area on its east or northeast side, viz, in Nos. 3, 6, 8, 9, 13, 18, 19, 20, 24, 25, 27, 28, 30, 32, 33, 34, 35, 37, 38, and 41. There are also four other cases which apparently ought to be included in the same class, but the Signal Service observations do not cover sufficient territory to furnish decisive information on this point. These cases are Nos. 1, 17, 39, and 40.

In twenty-two of the cases in Table XXX the low center advanced between two neighboring areas of high barometer, and its movement was apparently accelerated thereby, viz, in Nos. 2, 3, 4, 5, 6, 9, 12, 14, 15, 16, 17, 20, 21, 22, 23, 24, 31, 35, 36, 37, 42, and 43. It is probable that No. 26 should be included in the same class, and perhaps two or three other cases.

There are only eight cases not included in either of the preceding lists, viz, Nos. 1, 7, 10, 11, 26, 29, 39, and 40, and seven of the cases are included in both lists. There is little doubt that Nos. 1, 39, and 40 should be included in the first list, but the low center passed so near the northern boundary of the United States that the evidence is not entirely satisfactory. There are only four cases which do not apparently belong to one of the preceding classes, viz, Nos. 7, 10, 11, and 29, and in these cases the amount of rain-fall on the east side of the low center was unusually great, viz, an average rain-fall of 0.81 inch, 0.72 inch, 1.14 inch, and 0.93 inch, in 24 hours, for the entire area within which the barometer was below 30 inches. These are among the greatest rain-falls which have occurred in the United States since the Signal Service observations commenced. Greater rain-falls have occurred within districts of limited extent, but few cases have occurred which showed so large an average rain-fall for the entire extent of the low area.

In several of the cases in Table XXX the isobars were very much elongated in the direction towards which the low center advanced, so that a small change of pressure was sufficient to carry the low center forward with unusual rapidity. Nos. 4, 7, 10, 14, 17, 21, 23, 30, 34, 36, 37, 40, and 42 were of this kind, and if the stations of observation had been sufficiently extended to show in each instance the complete form of the isobars it is probable that more cases of the same kind

would have been found. In No. 17 the form of the isobars was quite similar to those shown on Plate 11.

From the last column in Table XXX we see that a considerable number of these storms were immediately succeeded by an area of high pressure of unusual magnitude. In thirteen of the cases the pressure exceeded 30.50 inches; in thirty-two of the cases the pressure was as great as 30.25 inches; but in six of the cases it was as low as 30.12 inches. These facts seem to indicate that an area of high pressure, immediately succeeding an area of low pressure, is favorable to the rapid progress of the latter, but a very high pressure is not essential to rapid progress.

79. We perceive that about four-fifths of the storms included in Table XXXI occurred between the Mississippi River and the Rocky Mountains, and they occurred most frequently in the neighborhood of Fort Sully and Bismarck. This region, therefore, appears to be especially favorable to the slow movement of areas of low pressure. A careful examination of the Signal Service maps shows that in the cases which occurred in the region above mentioned a pressure below 30 inches extended to a considerable distance westward, generally as far as the Pacific Ocean, and the low center did not make much progress eastward until an area of increased pressure, coming from the west or northwest, began to be felt on the east side of the Rocky Mountains. Nos. 1, 2, 3, 7, 9, 12, 13, 15, 18, 19, 24, 25, 26, 27, 28, 29, 33, 35, 36, 37, 38, 42, 44, 45, 46, 49, 51, and 52 were apparently examples of this kind.

In Nos. 5 and 11 there was an area of moderately high pressure prevailing at the time over the Rocky Mountains, but this high area remained sensibly stationary during the day in question. As soon as the high center began to advance eastward the low center advanced also, and at about the same rate.

In several instances the low areas appeared to have been filled up by a slowly increasing pressure on the north side, until the depression was so inconsiderable that it could not be satisfactorily traced. Nos. 8, 10, 20, 21, 22, 39, 40, 41, 43, and 48 were apparently of this kind.

In some of the cases the low center vibrated to and fro within the limits of a few hundred miles for two, three, or four days. At length an increasing pressure on the northwest side either drove the low center eastward or filled up the low area so that it could no longer be satisfactorily followed. Nos. 20, 35, 37, 41, 45, 46, and 51 were of this kind.

Apparently the reason why these areas of low pressure lingered so long in the neighborhood of Fort Sully and Bismarck was the absence of a sufficient pressure on the west side, and the Rocky Mountains apparently formed the barrier, which prevented the air from flowing in freely on the western side.

80. The amount of rain which accompanied these low areas was extremely small, and it is surprising that the winds within them acquired so great velocity. The average amount of rainfall in eight hours for the forty cases which occurred in the northwestern part of the United States was 0.06 inch on the east side of the low center and 0.02 inch on the west side. A part of this rain fell near the borders of the low area, where the pressure was but little less than 30 inches, and it probably had but little influence upon the movement of the low center. In the twenty-two cases which occurred nearest to Fort Sully there were only nine in which a drop of rain fell at that station during the days in question, and much of the time the sky was reported as either clear or fair.

81. It seems probable that the direct heat of the sun, acting upon the dry surface of the barren plains, between the meridian of 97° and the Rocky Mountains, supplied a large part of the moving force which maintained the velocity of the winds. Of the forty cases which the table enumerates for the northwestern part of the United States two occurred near the end of March, three occurred in April, ten in May, three in June, six in July, five in August, nine in September, and two in October. During five months, including the colder part of the year, no case occurred, and at the time of nearly all the cases here enumerated the heat was unusually great. Table XXXII shows for three successive days the maximum temperatures observed at Fort Sully at the time of the twenty-two cases which occurred nearest to that station. Column 1 shows the number of the case as recorded in Table XXXI; column 2 shows the maximum temperature two days before the date in the table; column 3 shows the maximum temperature one day before the given date, and column 4 shows the maximum temperature on the given day.

TABLE XXXII.—*Maximum temperatures observed at Fort Sully.*

No.	2 days before.	1 day before.	Given day.	No.	2 days before.	1 day before.	Given day.	No.	2 days before.	1 day before.	Given day.	No.	2 days before.	1 day before.	Given day.
	°	°	°		°	°	°		°	°	°		°	°	°
7	66	69	73	15	57	64	60	24	99	100	100	42	94	89	97
8	92	103	105	18	90	96	82	25	80	90	80	44	95	94	93
9	85	91	108	19	78	82	91	26	86	93	94	48	79	83	90
10	89	101	106	20	77	93	101	28	94	92	96	49	84	71	87
11	84	91	107	21	93	101	99	33	40	50	50	....	....	....	....
12	83	92	95	22	101	99	96	37	76	73	74	....	....	....	....

We see that in eight of these cases the thermometer rose to 100° and upwards; in thirteen of the cases the thermometer rose as high as 95°; and in seventeen cases it rose as high as 90°. Of the five cases in which the thermometer did not rise as high as 90° two occurred in March, two in May, and one in September. In three of these cases there was a considerable fall of rain at Fort Sully, and in the two remaining cases the temperature was above the average for that season of the year.

82. Besides the forty cases already examined Table XXXI contains twelve others, of which nine occurred near the northeastern portion of the United States; two occurred near the Gulf of Mexico; and one near Lake Erie. In all of these cases the most noticeable feature was a nearly stationary condition of the barometer throughout an extended region on the western side, reaching as far as the Rocky Mountains, and sometimes to the Pacific Ocean. In each case there was a second area of low pressure, at a distance generally of about 1,500 miles on the western side, which made very slow progress eastward; and in each of the cases (except Nos. 47 and 50) there was an area of high, or moderately high, pressure, generally situated between the two low areas, which high area was also nearly stationary for one or more days, or moved slowly towards the south without disturbing the low area on its eastern side. In the case of No. 14 this high area made no considerable progress for four days, and in Nos. 16 and 17 the high area remained nearly stationary for six days. These facts seem to indicate that the slow movement of these twelve low areas was not principally due to any local cause; it was not wholly due (probably it was not mainly due) to any thing occurring within the limits of the given low area; but a like stationary condition extended to a distance of several thousand miles on the western side, and must, therefore, have been the result of causes which had a very wide extent, perhaps comprehending a large portion of the northern hemisphere, and possibly portions of the southern hemisphere. We also perceive that the causes which determined the slow movement of these twelve low areas are in many respects similar to the causes which operated in the forty cases which occurred in the northwestern portion of the United States.

83. In order to study the movement of areas of low pressure under the greatest possible variety of circumstances, I have endeavored to obtain information from European observations. In the Uebersicht der Witterung for 1881, published by the Deutsche Seewarte, is given a table showing the mean velocity of movement of the barometric minima for the five years 1876–'80, as deduced from the monthly charts of storm tracks. The following table shows the average results deduced from the observations of these five years:

TABLE XXXIII.—*Rate of progress of storm centers in Europe.*

	Myriam. in twenty-four hours.	Miles per hour.	Miles in U. S.	Ratio.		Myriam. in twenty-four hours.	Miles per hour.	Miles in U. S.	Ratio.
January	67.3	17.4	33.8	1.94	August	54.1	14.0	22.6	1.61
February	69.4	18.0	34.2	1.90	September	66.7	17.3	24.7	1.43
March	67.6	17.5	31.5	1.80	October	73.2	19.0	27.6	1.45
April	62.6	16.2	27.5	1.70	November	72.0	18.6	29.9	1.60
May	56.9	14.7	25.5	1.73	December	69.3	17.9	33.4	1.87
June	60.9	15.8	24.4	1.54					
July	54.9	14.2	24.6	1.73	Year	64.6	16.7	28.4	1.70

Column 2 shows the velocity of movement for each month, expressed in myriameters for twenty-four hours; column 3 shows the velocity expressed in English miles per hour; column 4 shows the velocity of movement of storm centers for the United States, and column 5 shows the ratio of the numbers in columns 3 and 4.

We see that in the United States the average velocity of movement for the entire year is about two-thirds greater than it is in Europe. This ratio is greatest in winter, when it amounts to 1.9, and least in the autumn, when it amounts to 1.5. So large a difference between the mean ratio of progress of storm centers in the United States and Europe must be the result of a permanent cause of great energy. A comparison of the cases of most rapid movement in Europe with the cases of extremely slow movement may afford some clue to the nature of this cause.

84. The most satisfactory materials I have found upon which to base such a comparison are the daily weather charts, published by the Danish Government for three years, from December, 1873, to November, 1876, and by the Danish Government in connection with the Deutsche Seewarte, from December, 1880, to August, 1881. Table XXXIV contains the most decided cases of rapid motion that I have been able to find from a comparison of these charts. They are cases in which the depression of the barometer was considerable, and generally there was no second low center in the vicinity. The charts show a great number of other cases in which the movement of low centers apparently was equally rapid; but in some of them the exact position of the low

TABLE XXXIV.—*Atlantic and European storms advancing at least 750 miles in twenty-four hours.*

No.	Date.	First date.			Second date.			Progress.	Change of barometer in 24 hours.		High following.
		Latitude.	Longitude.	Barometer.	Latitude.	Longitude.	Barometer.		First station.	Second station.	
	1873.	°	°	*mm.*	°	°	*mm.*	*Miles.*	*mm.*	*mm.*	*mm.*
1	Dec. 16–17	61.3	1.1 W.	715	60.9	26.2 E.	715	966	+37	—29	770
	1874.										
2	Feb. 5– 6	66.7	20.9 E.	734	59.0	37.2 E.	733	759	+24	—22	775
3	13–14	68.7	11.8 E.	737	63.7	37.9 E.	730	780	+14	—20	765
4	Sept. 18–19	60.3	2.4 W.	738	64.3	20.7 E.	743	793	+14	—12	765
5	Nov. 1– 2	68.1	28.4 E.	724	64.0	55.0 E.	724	828	+28	—20	770
6	19–20	40.9	42.7 W.	730	62.2	55.7 W.	731	1566	+28	—24	765
	1875.										
7	Feb. 21–22	47.5	65.5 W.	730	60.1	59.7 W.	735	897	+35	—26	775
8	26–27	46.3	64.0 W.	726	56.2	53.7 W.	728	807	+26	—15	765
9	28–29	45.1	60.5 W.	730	60.4	52.5 W.	732	1097	+34	— 7	775
10	Mar. 1– 2	60.4	52.5 W.	732	73.3	45.0 W.	734	925	+14	—21	775
11	Oct. 4– 5	59.6	29.9 W.	735	65.3	6.5 W.	725	862	+20	—21	775
	1876.										
12	Jan. 11–12	49.8	60.4 W.	735	60.8	53.4 W.	732	800	+12	—21	775
13	20–21	48.0	65.6 W.	731	59.2	49.1 W.	715	1021	+26	—35	775
14	27–28	63.8	33.9 W.	724	49.2	34.2 W.	716	1007	+15	—34	770
15	28–29	49.2	34.2 W.	716	61.8	32.6 W.	720	869	+32	—18	770
16	Apr. 5– 6	69.3	1.1 W.	730	66.3	39.5 E.	736	1111	+22	—17	770
17	11–12	57.2	8.6 E.	723	63.4	27.2 E.	727	766	+24	—18	775
18	July 21–22	60.5	54.3 W.	732	75.8	53.3 W.	731	1056	+18	—23	770
19	22–23	75.8	53.3 W.	731	71.8	19.6 W.	734	787	+13	—18	765
20	Oct. 12–13	60.7	2.4 E.	721	69.7	32.5 E.	726	1069	+35	—17	765
	1880.										
21	Dec. 14–15	56.0	14.0 E.	737	50.8	34.5 E.	730	883	+20	—19	770
22	30–31	47.5	61.0 W.	727	64.0	54.5 W.	707	1145	+26	—28	770
	1881.										
23	Jan. 16–17	36.4	27.5 W.	737	43.3	11.1 W.	740	980	+22	—17	765
24	17–18	45.3	35.7 E.	743	55.8	47.7 E.	736	883	+26	—22	770
25	20–21	52.2	11.2 E.	738	50.0	39.4 E.	733	1063	+24	—26	765
26	21–22	50.0	39.4 E.	733	59.4	61.1 E.	731	1056	+17	—25	765
27	Feb. 13–14	45.6	68.8 W.	735	60.0	51.0 W.	731	1203	+23	—15	775
28	Apr. 26–27	45.5	46.9 W.	738	58.5	43.9 W.	746	904	+24	— 8	775
29	May 16–17	56.4	1.3 E.	737	66.9	16.1 E.	737	862	+21	—18	770
	Means..	....	....	731.0	....	....	729.5	956	+23.2	—20.6	770

center is not clearly indicated, and in other cases the change in the form of the isobars in twenty-four hours was so great as to leave some doubt respecting the identity of the low areas. With regard to the rapid movement of the twenty-nine cases included in the table it is thought there can be no difference of opinion.

Column 1 gives the number of reference; column 2, the dates of the two maps compared; columns 3 and 4 show the latitude and longitude of the low center at the first of the two dates; column 5 shows the estimated height of the barometer in millimeters at the center of the low area. This estimated pressure is generally two or three millimeters less than that of the lowest isobar drawn on the map. Columns 6 and 7 show the latitude and longitude of the center of the low area at the second of the two dates, and column 8 shows the estimated pressure at the center; column 9 shows the progress of the low center in twenty-four hours expressed in English miles; column 10 shows the rise of the barometer at the first-named point during the twenty-four hours succeeding the first dates, and column 11 shows the fall of the barometer at the second-named point during the twenty-four hours preceding the last named date. Generally the low area was immediately succeeded by an area of pressure above 760^{mm}. Column 12 shows the highest isobar in this area. The highest pressure was probably a few millimeters greater than the highest isobar. At the bottom of the table are given the average values of the numbers in six of the columns.

85. We see from this table that the depression at the center of the low areas increased slightly during the given twenty-four hours, showing a slight increase in the intensity of the storms. The average rise of the barometer at the first station during the succeeding twenty-four hours was 23.2^{mm}, or 0.91 inch, and the average fall of the barometer at the second station during the same time was 20.6^{mm}, or 0.81 inch. These changes are nearly one-half greater than those shown in Table XXX for American storms.

If now we examine each of these cases singly we shall find that in about half of them, viz, Nos. 4, 9, 11, 12, 13, 15, 16, 17, 22, 23, 24, 25, 26, and 27, there was a second low center nearly in the direction towards which the first low was moving, and this may be supposed to have accelerated the movement of the first low center. The same was probably true in several cases not distinctly indicated by the charts, and in all of the cases the point reached by the low center at the end of twenty-four hours (if not previously the center of a system of circulating winds) was at least a point where the pressure was less than 760^{mm}, and the barometric gradient was very feeble. No. 14 may perhaps be claimed as an exception to this remark, but if we had a chart for the evening of January 27 it might perhaps appear that the low area prevailing in the middle of the Atlantic Ocean on the morning of January 28 is to be connected with the low area prevailing south of Hudson's Bay on the morning of January 27.

In more than a third of the cases, viz, Nos. 6, 7, 9, 10, 12, 15, 16, 17, 23, 25, and 26, the low center advanced between two neighboring areas of high barometer, and its movement may have been thereby accelerated. The same was probably true in some cases not indicated by the maps, since the low areas enumerated in the table were generally near the northern limit of the charts. It must, however, be admitted that in many of the cases the charts do not show two such areas of high barometer. In all of the cases the charts show a fall of rain or snow on the front side of the low center, but I have not been able to make any satisfactory estimate of its amount. As the low center moved forward it was succeeded by a pressure above 760^{mm}, as is shown by column 12 of the table. The average of the highest isobars, following the low centers, was 770^{mm}, and the average pressure at the center of the high areas was about 772^{mm}, or 30.39 inches, the same as found in Table XXX.

86. Table XXXV shows the most distinctly marked cases in which a storm center, over the Atlantic Ocean or Europe, has advanced not more than 200 miles in twenty-four hours. The arrangement of this table is similar to that of Table XXXIV.

We see from this table that during the twenty-four hours here compared the depression at the center of the low area in some cases increased, and in other cases decreased, but generally the change did not exceed two or three millimeters. On an average of the fifty cases the depression at the center was slightly diminished. In forty of the cases there was a slight increase of pressure at the first station during the twenty-four hours here considered, and in thirty-two of

the cases there was a slight decrease of pressure at the second station. In nine of the cases there was a diminution of pressure at the first station, but during the same time there was a greater diminution of pressure at the second station, showing that the storm was increasing somewhat in intensity. In twelve of the cases there was a slight increase of pressure at the second station, but during the same time there was an equal or greater increase at the first station, showing that the intensity of the storm was decreasing.

TABLE XXXV.—*Atlantic and European storms advancing not more than 200 miles in twenty four hours.*

No.	Date.		First date.			Second date.			Progress	Change of barometer in twenty-four hours.	
			Latitude.	Longitude.	Barometer.	Latitude.	Longitude.	Barometer.		First station.	Second station.
	1874.		°	°	*mm.*	°	°	*mm.*	*Miles.*	*mm.*	*mm.*
1	Mar.	24–25	58.6	47.4 W.	731	59.6	49.1 W.	721	97	− 8	−13
2	May	29–30	60.7	52.2 W.	736	59.4	47.3 W.	737	193	+ 5	− 4
3	June	1– 2	56.4	50.6 E.	742	57.6	52.9 E.	740	124	+ 1	− 5
4	July	11–12	52.3	52.4 E.	746	51.5	52.0 E.	742	69	− 1	− 5
5	Aug.	11–12	56.7	1.1 W.	742	57.5	0.3 W.	743	55	+ 2	− 1
6	Sept.	5– 6	64.3	11.9 E.	742	63.9	16.6 E.	740	152	+ 1	− 3
7	Oct.	2– 3	57.7	9.5 W.	719	59.4	5.1 W.	719	193	+10	− 9
8		9–10	66.2	24.8 W.	723	63.8	26.2 W.	723	166	+ 9	− 6
9	Nov.	11–12	63.3	31.7 E.	722	63.1	34.9 E.	736	103	+15	+11
10		25–26	61.4	24.9 W.	734	62.3	26.9 W.	735	97	+ 3	− 1
11	Dec.	12–13	51.9	1.1 E.	729	51.2	5.6 E.	735	200	+13	0
12		16–17	40.5	53.3 W.	741	41.4	53.5 W.	736	62	− 3	− 8
13		30–31	58.5	50.0 W.	722	59.4	51.2 W.	716	69	− 5	− 6
	1875.										
14	Jan.	7– 8	51.8	32.7 W.	718	49.8	30.1 W.	715	186	+ 5	−10
15		9–10	53.4	27.4 W.	725	51.6	27.7 W.	725	124	+ 3	− 4
16	Mar.	13–14	49.0	38.1 W.	730	50.2	37.2 W.	734	90	+ 8	0
17	Apr.	17–18	45.5	26.9 W.	736	46.5	27.5 W.	740	83	+ 5	+ 1
18	May	20–21	60.2	30.4 W.	720	58.2	27.5 W.	720	179	+ 5	−10
19		22–23	62.5	23.4 W.	724	64.7	25.2 W.	737	166	+16	+ 8
20		23–24	64.7	27.2 W.	737	63.6	20.0 W.	742	179	+ 9	+ 1
21	Aug.	29–30	61.1	51.7 W.	740	59.5	55.0 W.	740	159	+ 5	− 5
22		30–31	59.5	55.0 W.	740	60.0	51.5 W.	738	62	0	− 7
23	Sept.	20–21	72.8	30.6 E.	726	71.3	32.5 E.	721	117	+ 4	− 9
24		22–23	45.5	28.6 W.	730	44.8	25.2 W.	735	179	+11	− 4
25		28–29	61.0	52.7 W.	736	60.0	55.5 W.	737	114	+ 4	− 2
26	Oct.	18–19	60.3	54.6 W.	741	61.5	55.3 W.	732	83	− 6	−11
27	Dec.	17–18	64.3	29.6 W.	718	63.0	30.3 W.	721	97	+ 6	+ 1
28		30–31	65.6	28.9 W.	729	67.0	24.2 W.	727	166	+ 1	− 4
	1876.										
29	Jan.	21–22	59.2	49.1 W.	715	61.0	51.2 W.	711	131	+ 2	− 9
30	Feb.	1– 2	64.2	27.5 W.	716	64.5	23.2 W.	719	131	+ 9	− 2
31		5– 6	61.2	50.2 W.	739	63.8	51.1 W.	722	183	−10	−23
32	Mar.	9–10	59.4	2.3 W.	710	58.6	1.0 W.	710	69	+ 4	− 2
33	May	1– 2	43.9	66.1 W.	741	44.9	63.4 W.	743	159	+ 6	− 4
34	June	14–15	58.3	18.1 W.	729	59.5	14.7 W.	731	152	+11	− 7
35		18–19	57.4	27.6 W.	736	57.6	23.4 W.	725	159	− 3	−19
36	July	1– 2	64.1	24.6 W.	731	65.9	19.5 W.	735	200	+ 9	0
37		23–24	71.8	19.6 W.	735	70.1	19.8 W.	735	117	+ 3	− 1
38	Sept.	11–12	59.4	8.9 E.	735	59.6	7.0 E.	740	62	+ 8	+ 3
39		22–23	54.4	26.7 W.	735	56.0	29.7 W.	737	159	+ 7	− 2
40	Oct.	8– 9	57.4	50.6 W.	724	58.3	49.5 W.	718	65	− 4	−12
41		12–13	55.9	49.6 W.	715	57.3	49.3 W.	720	97	+10	0
42	Nov.	12–13	48.0	19.6 W.	728	47.3	18.5 W.	722	69	− 2	−12
	1880.										
43	Dec.	21–22	66.3	12.5 E.	737	67.0	12.0 E.	739	48	+ 3	0
	1881.										
44	Jan.	5– 6	46.7	37.5 W.	715	46.6	37.5 W.	727	7	+12	+12
45		6– 7	46.6	37.5 W.	727	46.9	36.9 W.	735	48	+10	+ 7
46		18–19	49.0	2.0 W.	733	49.5	1.6 W.	737	34	+ 5	+ 4
47	Mar.	16–17	66.3	24.4 W.	733	64.4	22.5 W.	744	131	+15	+ 4
48		27–28	47.0	59.7 W.	728	47.0	59.7 W.	730	0	+ 2	+ 2
49	Apr.	20–21	58.4	18.8 E.	730	58.5	18.5 E.	736	11	+ 6	+ 6
50	May	18–19	56.3	12.6 W.	737	56.3	10.5 W.	738	76	+ 3	0
			....	....	730.1	....	....	730.8	113	+ 4.5	− 3.4

87. The cases enumerated in this table are all comprehended between latitude 40° and latitude 72°, and between longitude 66° W. and 52° E., but they are by no means uniformly distributed over this area. Eleven cases occurred near the southern extremity of Greenland, viz, Nos. 1, 2, 13, 21, 22, 25, 26, 29, 31, 40, and 41. Nine cases occurred near the western coast of Iceland, viz, Nos. 8, 10, 19, 20, 27, 28, 30, 36, and 37. These cases seem to indicate distinctly the influence of local causes. Both of these localities are remarkable for a larger rainfall than is generally found in the same latitude, and it seems probable that this rainfall exerted an influence to hold the low center for twenty-four hours in a nearly fixed position. A similar remark applies to Nos. 5, 7, and 32, near Scotland; to Nos. 6 and 43, on the coast of Norway; to No. 33, near Nova Scotia; and No. 48, near Newfoundland. In all of these cases (twenty-seven in number) it seems probable that local causes exerted an appreciable influence on the movement of the low center, and there are a few other cases in which there is room to suspect the influence of local causes.

88. There remains, however, a large number of cases situated in the middle of the Atlantic Ocean which we cannot ascribe to any local influence, and therefore it may be presumed that in the cases above specified local influence was not the sole cause of the slow movement of the low centers.

It seems impossible to avoid the conclusion that the extremely slow progress of storm centers which we sometimes observe, and the extremely rapid progress which we find at other times, are mainly due to variations in that general movement of the atmosphere which is shown in the average system of atmospheric circulation. It has already been stated that there are permanent causes in operation, which, in the middle latitudes, give rise to an average wind from west to east. The operation of these causes is temporarily suspended by the action of great storms, which give rise to easterly winds, but the permanent causes which influence the winds are not changed by the action of storms, however violent. By temporary obstruction the permanent causes acquire increased energy. Hence it results that the general system of circulation of the winds, although pretty uniform when we compare the average direction of one year with another, appears very irregular when we compare one day with another. Sometimes over large portions of the earth's surface the movement of the winds from west to east goes on with destructive violence; sometimes, over extended districts, the movement is reversed, and the prevalent wind is from east to west; while at other times the advance from west to east is almost entirely suspended, or proceeds in the average direction with inconsiderable velocity. According to this view the general system of atmospheric circulation (consisting of the trade winds of the equatorial regions and the prevalent westerly winds of the middle latitudes) is the primary cause which determines both the direction and velocity of the movement of storm centers; but for each individual storm the determining cause is not so much the average system of atmospheric circulation as the general movement of the atmosphere which is going on at the time and in the vicinity of that particular storm. The influence of this general movement is moreover materially modified by a variety of causes, such as the amount of rainfall and the position of the rain-areas with reference to the center of the storm, the magnitude and position of the neighboring areas of high and low pressure, the distribution of temperature, local influences, &c.

89. The preceding investigation has shown that the causes which produce unusually rapid movements or unusually slow movements of storm centers in Europe are similar to the causes which produce like results in the United States, but it does not explain why the average movement of storm centers in the United States is so much greater than it is in Europe. In the hope of obtaining some light on this question I have determined the average velocity of storm centers over the Atlantic Ocean by a comparison of the monthly charts of storm tracks published with the International Bulletin for a period of four years from 1879 to 1882. The following table shows in miles per hour the average rate of progress for each month of the year:

Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
17.4	19.5	19.7	19.4	16.6	17.5	15.8	16.3	17.2	18.7	20.0	18.3

The average velocity for the entire year is 18 miles per hour.

90. If now we compare the preceding results with those heretofore found for the West India cyclones, while pursuing a westerly course, and for the cyclones of the Bay of Bengal and China Sea, for the same part of their course, we shall have a view of the movement of storm areas under a great variety of conditions. The average results for these five districts for the entire year are as follows:

	Miles per hour.
For the United States	28.4
Middle latitudes of the Atlantic Ocean	18.0
Europe	16.7
Neighborhood of the West Indies	14.7
Bay of Bengal and China Sea	8.5

The velocity here given for the West India cyclones is the mean of all the determinations in Tables III and IV, and the velocity given for the Bay of Bengal and China Sea is the mean of all the determinations in Tables VII and IX.

Thus we see that the average rate of progress of storm centers over the Atlantic Ocean is about the same as over Europe and is double the rate of progress for the China Sea, and the rate of progress for the United States is more than three times the rate for the China Sea. These results are derived from so large a number of observations that they must be accepted as substantially correct, and they demand a clear explanation.

91. I have endeavored to determine how far these differences may result from a difference in the mean velocity of the wind for these several districts. For this purpose I determined the average velocity of the wind for that portion of the United States within which the storm centers are most frequently found, viz, that portion north of the parallel of 40° and east of the meridian of 100° from Greenwich. A slight examination of the observations shows that at stations near the Atlantic Ocean or near one of the Great Lakes the velocity of the wind is greater than at stations in the interior of the country. I have therefore divided the observations into two groups, one including the stations near the ocean or one of the Great Lakes, and called coast stations, the other group including the remaining stations, which are called inland stations. Table XXXVI shows for each month of the year the average monthly movement of the wind in miles for these two classes of stations, according to the Annual Report of the Chief Signal Officer for 1883.

Column 2 shows for each station the number of years of observation, and at the bottom of each group of stations is given the mean hourly velocity of the wind for that group. In the succeeding line is given the mean between the velocities of the two groups; the next line shows for each month the rate of progress of storm centers as given in Table XXIX, and the last line shows the ratio of the velocity of storm centers to the mean velocity of the wind.

We see that this ratio is not the same for all months, but for that month in which the rate of progress of storms is greatest the ratio is sensibly the same as for that month in which the rate is the least. This coincidence seems to indicate that the rate of progress of storms is in some degree dependent upon the mean velocity of the wind, but the considerable inequalities in the value of the ratio show that the rate of progress of storms cannot depend solely on the average velocity of the wind.

92. I next determined, as well as I was able with the means at my command, the average velocity of the wind for that part of Europe within which storm centers are most frequently found, viz, between the parallels of 50° and 60°. Table XXXVII shows the results which I have obtained, the velocities being expressed in meters per second, and the observations are divided into two groups, as in Table XXXVI.

Column 4 shows the number of years of observations from which the velocities are derived. At the bottom of each group of stations is given the mean of the observations for that group. The succeeding line shows the average between the results for the separate groups; the next line shows the mean velocities expressed in miles per hour; the next line shows the average rate of progress of storm centers, as given in Table XXXIII; and the last line shows the ratio of the numbers in the two preceding lines. These ratios are quite different from those found for the United States, and the correspondence between the rate of storm movements and the movements of the wind is not as distinctly marked. Nevertheless some degree of correspondence can be detected, and it is noticeable that the change in the wind's mean velocity for the different months

TABLE XXXVI.—*Mean monthly movement of the wind for the United States.*

COAST STATIONS.

	Years.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Alpena	10	6706	6809	7747	6689	6072	5737	5493	5171	6031	6653	6833	7042
Block Island	2	13181	11236	13169	11207	10694	8807	7821	6939	10364	10955	12351	12740
Boston	12	7225	7212	8342	7283	6454	5522	5377	5166	5886	6501	7256	7340
Buffalo	12	7961	7006	7565	6233	5580	5057	5183	4647	5711	6646	7534	8798
Chicago	12	6646	6219	7406	6812	6393	5549	5256	5201	5668	6325	6177	6469
Cleveland	12	8011	6956	8035	6712	6125	5712	5404	5103	6349	7378	8040	8121
Duluth	12	4740	5119	5740	6142	4910	4437	4805	4859	5383	5826	5197	5140
Eastport	10	9227	8608	9503	7425	6281	4995	4700	4424	5506	7096	8735	8793
Erie	10	8922	8047	8670	8043	6533	6138	5391	5188	6390	7762	9004	9596
Escanaba	12	7198	6924	8101	7482	6759	6260	6219	5776	6797	7739	6894	7196
Grand Haven	12	8535	8266	9196	8502	7828	6499	6236	5930	7113	8785	8624	8894
Marquette	12	6863	6041	6946	5986	5323	4867	5097	5124	6203	6892	6574	7083
Milwaukee	12	8601	8206	9522	8383	7698	6719	6220	6584	7240	8316	8528	9017
New Haven	10	5760	5634	7130	6602	5492	4628	4444	4317	4914	5425	5876	5846
Newport	7	8316	7473	8167	7162	6037	5121	5053	4754	5546	7041	7973	8535
New York	12	7496	7506	8645	7251	6463	5813	5740	5538	6418	6884	7465	7659
Oswego	12	7797	7199	7682	6397	5481	4537	4584	4269	5248	6324	7432	7890
Port Huron	8	7501	7472	8130	7732	7268	6075	5720	5348	6144	7095	7352	7810
Portland	12	5560	5607	6721	6426	5778	4931	4841	4250	4843	5476	5956	5810
Rochester	12	8095	7347	8424	7476	6940	5971	5593	5102	6863	6608	7118	7901
Sandusky	5	9759	9716	10999	10223	9414	8689	7652	7615	8440	9624	10306	10126
Sandy Hook	9	10705	9875	12375	10418	8891	8351	7835	8206	9744	10842	11821	12565
Thatcher's Island	7	12773	12046	12783	10197	9086	7437	7061	6817	8413	9809	11528	12492
Toledo	12	7073	6187	7306	6647	6604	5667	5015	4842	5229	6244	6546	6992
Wood's Holl	10	9554	9258	10228	8715	7422	6525	6512	6391	6222	8062	9788	10417
Hourly	......	10.98	11.33	11.76	10.67	9.22	8.34	7.70	7.42	8.98	9.96	11.16	11.30

INLAND STATIONS.

	Years.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Albany	9	6029	5816	6776	6310	5274	4265	3793	3316	3643	4454	5223	5565
Breckenridge	11	9323	8782	10715	9924	10036	8388	7571	7314	7862	9050	8655	8880
Burlington	12	6069	4996	6003	5537	4999	4434	4255	3918	4801	5366	6176	6246
Champaign	2	8368	8160	12266	9580	8796	8219	6831	6282	7476	7398	8392	9208
Davenport	12	6206	6001	7712	7518	6732	5545	4885	4715	5504	5987	6202	6146
Des Moines	4	4527	5094	6168	5831	4820	4540	3576	3378	4117	4406	4767	4776
Detroit	12	5890	5718	6703	6119	5674	4927	4676	4408	4821	5662	5773	6134
Dubuque	10	3609	3849	5030	5095	4484	3918	3186	3152	3341	3985	3819	3552
Huron, Dak	2	8829	7679	11300	10010	8966	6616	8146	7249	7615	7702	8052	7147
Keokuk	11	6069	5770	7160	7291	6664	5490	4501	4649	5380	5846	5967	5764
La Crosse	9	4986	5329	6281	6153	5733	5171	4620	4498	5030	5806	5334	5035
Madison	4	7487	8179	9141	8053	7230	6184	5501	5686	6402	7575	7765	7670
Moorhead	2	7677	7822	10215	8432	8930	7165	8646	7713	7423	7784	8298	8314
New London	12	5708	5928	6912	6276	5338	4417	4259	4043	4500	5330	5846	5594
Omaha	12	6893	6465	8178	7840	7134	6098	5450	5363	5821	6552	7084	6551
Pittsburgh	12	5073	4603	5652	4834	4068	4059	3773	3281	3652	4086	4737	5174
Saint Paul	12	5945	5673	7042	6845	7174	6250	5401	5488	5950	6711	6062	5753
Saint Vincent	2	7338	7872	8359	7571	7712	6186	6864	5747	6171	7477	7859	7281
Springfield, Mass.	10	3812	3874	4789	4605	4043	3485	3071	2709	3103	3483	3812	3834
Williamsport	1	5135	3764	5071	4245	3489	3495	3095	2422	2447	2036	2645	5025
Hourly	....	8.40	8.94	10.18	9.59	8.55	7.56	6.87	6.40	6.96	7.84	8.50	8.20
Mean hourly	......	9.69	10.13	10.97	10.13	8.88	7.95	7.28	6.91	7.97	8.90	9.83	9.75
Storms	......	33.8	34.2	31.5	27.5	25.5	24.4	24.6	22.6	24.7	27.6	29.9	33.4
Ratio	......	3.5	3.4	2.9	2.7	2.9	3.1	3.4	3.3	3.1	3.1	3.0	3.4

of the year in Europe is only about half as great as in the United States. The inequalities in the value of the ratio for the different months are considerable, and indicate the operation of some other cause than the mean velocity of the wind.

93. I next determined, as well as I was able with the means at my command, the average velocity of the wind in the neighborhood of the Bay of Bengal and China Sea. Table XXXVIII shows the results which I have obtained, the observations being all derived from the Report on the Meteorology of India for 1882, with the exception of Manilla, which is derived from the international observations. I have employed only stations south of latitude 20°, and I have rejected all stations having an elevation greater than 3,000 feet.

TABLE XXXVII.—*Mean velocity of the wind in Northern Europe.*

COAST STATIONS.

	Lat. °	Lat. ′	Long. °	Long. ′	Years.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Liverpool	53	25	3	0 W.	5	6.53	6.39	5.36	5.72	5.10	5.32	5.27	5.10	4.78	5.50	4.78	7.60
Greenwich	51	29	0	0	10	5.20	5.52	5.45	4.92	4.54	4.50	4.36	4.42	4.30	4.68	5.12	5.26
Borkhum	53	35	6	40 E.	6	5.32	5.40	6.13	5.54	5.18	4.98	5.19	5.58	4.81	6.06	6.77	5.79
Wilhelmshaven	53	30	8	8	7	7.55	8.23	7.89	7.50	6.79	6.25	6.16	6.48	6.04	7.53	8.17	7.90
Keitum	54	54	8	22	8	5.47	5.93	6.07	5.59	5.22	4.98	4.94	5.30	4.95	5.95	6.29	5.42
Hamburg	53	33	9	58	8	6.28	6.19	5.92	5.36	4.83	4.79	5.18	5.26	4.84	5.64	6.65	6.13
Kiel	54	20	10	9	4	6.92	7.24	7.05	5.81	6.08	5.55	5.63	6.01	5.19	6.31	7.14	5.92
Wustrow	54	21	12	24	6	7.64	7.09	6.99	5.98	5.68	5.63	6.14	6.23	5.83	6.87	8.38	7.23
Swinemünde	53	56	14	17	8	5.59	5.47	5.47	5.42	4.9	4.21	4.35	4.89	4.79	5.66	6.28	5.82
Neufahr Wasser	54	24	18	40	8	4.70	4.84	4.93	4.31	4.33	3.53	3.68	3.84	3.75	4.50	4.72	4.46
Libau	56	31	21	1	7	6.49	6.26	6.29	5.35	5.85	5.10	5.72	5.69	6.35	6.57	7.00	6.43
Memel	55	43	21	7	6	7.07	6.49	5.81	4.43	5.16	4.58	5.18	5.45	5.03	6.24	5.85	6.41
Mean						6.20	6.25	6.08	5.49	5.30	4.95	5.15	5.35	5.06	5.96	6.45	6.17

INLAND STATIONS.

	Lat. °	Lat. ′	Long. °	Long. ′	Years.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Oxford	51	46	1	16 W.	18	5.65	5.44	5.47	4.80	4.25	4.21	4.05	4.24	4.35	4.23	4.66	4.97
Ebersdorf	51	0	16	0 E.	6	4.51	5.07	4.11	4.17	4.22	2.82	2.74	2.79	3.20	4.17	4.82	4.88
Upsala	59	52	17	38	11	4.13	4.03	3.97	3.91	4.10	3.79	3.21	3.10	3.22	3.76	3.81	3.59
Cracow	50	4	19	58	3	1.92	2.61	2.61	2.36	2.25	1.64	1.94	1.31	1.61	2.31	2.25	2.28
Warschau	52	13	21	2	8	4.60	4.75	4.82	4.14	4.00	3.24	3.41	3.28	3.64	4.28	4.91	4.80
Wilna	54	41	25	18	8	1.74	1.87	2.14	1.75	2.24	1.71	2.00	1.55	1.57	1.81	1.82	1.67
Pinsk	52	7	26	6	8	4.83	4.94	5.32	4.54	3.91	3.34	3.24	3.24	3.53	4.09	4.44	5.21
Dorpat	58	23	26	43	8	3.27	3.51	3.41	3.25	3.15	2.90	2.53	2.56	2.65	3.09	3.25	3.01
Staryj Bye	53	31	30	16	5	4.45	4.61	5.43	3.81	4.10	3.66	3.41	3.42	3.33	3.77	4.62	4.59
St. Petersburg	59	56	30	16	8	4.18	4.46	4.40	3.88	3.75	3.70	3.36	3.40	3.84	4.09	4.53	4.10
Kiew	50	27	30	30	8	3.46	3.84	4.34	3.36	3.12	2.88	2.95	3.06	2.94	3.24	3.25	3.91
Nowgorod	58	31	31	18	4	4.16	4.71	4.80	3.65	3.94	3.72	3.11	2.90	2.96	3.48	4.29	3.58
Moskau	55	50	37	33	8	4.19	4.36	4.43	3.93	3.39	3.44	3.00	3.52	3.56	4.18	4.29	4.46
Kasan	55	47	49	8	8	2.86	2.87	3.12	2.80	2.52	2.17	1.87	1.84	2.08	2.75	3.12	2.99
Mean						3.85	4.08	4.17	3.60	3.50	3.09	2.92	2.87	3.03	3.52	3.86	3.86
General mean						5.02	5.16	5.12	4.54	4.40	4.02	4.03	4.11	4.05	4.74	5.15	5.01
Miles per hour						11.23	11.74	11.45	10.15	9.84	8.99	9.01	9.19	9.06	10.60	11.52	11.21
Storms						17.4	18.0	17.5	16.2	14.7	15.8	14.2	14.0	17.3	19.0	18.6	17.9
Ratio						1.5	1.6	1.5	1.6	1.5	1.8	1.6	1.5	1.9	1.8	1.6	1.6

Column 4 shows the height of the stations in English feet, and column 5 shows the number of years of observation employed. The numbers given for each month represent the mean daily movement of the wind in English miles. At the bottom of each group of stations is given the mean of the numbers in each column of that group; the following line gives the average of the numbers in the two groups; the next line shows the velocity of the wind expressed in miles per hour; the next line shows the average rate of progress of the cyclones recorded in Tables VII and IX for each month in which more than one cyclone was observed, and the last line shows the ratio of the numbers in the two preceding lines.

Here we find no correspondence between the average rate of progress of storm centers for the different months of the year and the average velocity of the wind, the rate of progress of storms being no greater for the four months in which the wind was strongest than for the four months in which the wind was feeblest. The inequality in the values of the ratio for the different months is quite noticeable, but this may be partly due to the small number of the observations.

94. I next endeavored to determine the mean velocity of the wind in the neighborhood of the West India Islands, but found very few observations suited to this purpose. Table XXXIX shows all the materials I have been able to obtain. The numbers for the first four stations are derived from the Signal Service observations, the numbers for the last two stations are derived from the international observations, and the numbers for Havana are derived partly from the international observations and partly from observations at the observatory of St. Belen.

TABLE XXXVIII.—*Mean daily movement of the winds in Southern Asia.*

COAST STATIONS.

	Lat. °	Lat. ′	Long. °	Long. ′	Elev.	Years.	Jan.	Feb.	Mar.	Apr.	May	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Bombay.	18	54	72	49	37	13	215	258	278	271	214	396	458	320	285	231	236	234
Vizagapatam ..	17	42	83	22	31	11	41	51	76	96	94	94	101	76	56	50	61	53
Madras	13	5	80	17	22	13	149	123	154	193	226	218	204	175	156	122	164	182
Mangalore	12	52	74	54	52	3	91	90	89	92	106	89	78	73	58	69	77	95
Negapatam. ...	10	46	79	53	15	12	136	97	93	127	184	199	185	152	134	99	135	170
Jaffna.	9	40	79	56	9	12	75	73	96	155	277	322	293	284	275	175	72	81
Trincomalee ...	8	33	81	15	175	12	231	190	168	221	334	469	444	372	362	253	159	202
Batticaloa	7	43	81	44	26	12	235	206	161	151	137	135	143	134	123	126	112	174
Colombo	6	56	79	52	40	13	180	135	113	126	188	233	203	194	195	167	126	177
Hambantota ...	6	7	81	7	40	12	244	237	183	181	271	282	291	288	282	218	155	205
Galle....	6	1	80	14	40	10	56	54	64	89	185	236	194	201	205	179	83	68
Moulmein	16	29	97	40	94	3	58	58	71	74	61	72	83	64	64	41	57	58
Diamond Island.	15	52	94	19	41	4	157	179	192	173	161	203	211	180	164	163	199	168
Mergui	12	11	98	38	96	5	50	52	50	55	59	56	53	47	40	36	41	44
Port Blair......	11	41	92	2	61	7	173	122	112	132	186	258	289	244	236	163	175	168
Nancowry......	8	0	93	46	81	9	227	205	150	121	150	263	273	251	240	149	129	168
Manilla	14	35	120	56	54	1	89	101	129	79	117	199	223	266	173	137	95	95
Mean	....		....		......	......	144	131	128	137	172	219	219	199	179	139	126	137

INLAND STATIONS.

	Lat. °	Lat. ′	Long. °	Long. ′	Elev.	Years.	Jan.	Feb.	Mar.	Apr.	May	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Chanda..... ..	19	56	79	19	652	12	48	65	76	91	115	147	187	121	88	54	41	38
Sironcha.......	18	51	80	0	401	7	48	70	85	112	121	112	105	84	71	69	66	50
Poona	18	28	74	10	1849	3	125	165	198	241	334	405	310	338	278	135	132	140
Sholapur	17	41	75	56	1590	4	154	150	161	173	237	269	260	197	198	139	158	142
Secunderabad..	17	27	78	33	1787	6	97	86	117	126	165	309	327	238	156	84	77	71
Belgaum	15	52	74	42	2550	6	92	81	102	101	167	185	186	174	146	94	100	101
Bellary	15	9	76	57	1455	10	82	98	113	128	200	243	276	254	217	103	78	75
Bangalore	12	59	77	38	2981	12	79	75	77	74	120	189	192	169	150	87	82	84
Salem	11	39	78	12	940	10	137	150	146	129	115	135	127	110	96	69	87	110
Coimbatore	11	0	77	0	1348	13	79	80	82	88	133	196	207	184	165	87	65	76
Trichinopoly...	10	50	78	44	275	12	123	105	99	105	155	251	264	202	169	96	109	126
Madura	9	55	78	10	448	13	131	123	101	87	91	124	113	91	85	72	99	122
Kandy	7	18	80	40	1696	12	75	77	48	34	54	82	74	69	63	44	43	55
Thyetmio	19	22	95	12	134	4	62	120	111	202	199	208	164	152	120	89	82	95
Tounghoo......	18	57	96	24	169	4	48	49	75	96	103	118	114	99	67	52	45	64
Rangoon	16	46	96	12	41	6	104	1 0	125	145	122	132	137	123	98	80	103	122
Bassein	16	4	94	50	35	4	57	86	107	116	101	98	89	84	83	60	65	55
Mean	....		....		......	......	91	99	109	120	149	188	184	158	132	83	84	90
General mean ..	....		....		......	......	117	115	118	128	160	203	201	178	155	111	105	113
Miles per hour .	....		....		......	......	4.87	4.79	4.91	5.34	6.67	8.46	8.37	7.42	6.46	4.63	4.37	4.71
Storms.....	....		....		......	......	.....	.....	.....	7.54	8.54	5.62	8.36	10.3	9.77	9.26	7.38	....
Ratio	....		....		......	......	.....	.....	.....	1.4	1.3	0.7	1.0	1.4	1.5	2.0	1.7	....

TABLE XXXIX.—*Mean velocity of the wind near the West India Islands.*

	Lat. °	Lat. ′	Long. °	Long. ′	Years.	Jan.	Feb.	Mar.	Apr.	May.	June	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Punta Rassa	26	36	82	10	12	10.15	10.80	12.06	12.20	10.51	8.92	8.16	8.26	8.72	11.06	10.09	10.31
Rio Grande City.....	26	22	98	45	8	6.94	7.84	7.69	9.31	9.94	10.00	10.67	7.32	6.81	6.17	6.81	6.08
Brownsville	25	53	97	26	7	6.80	7.61	7.93	8.49	7.29	7.17	6.78	4.90	4.37	5.00	7.36	6.75
Key West	24	34	81	49	12	10.79	10.18	11.28	10.62	9.59	7.67	7.51	7.39	8.30	11.92	11.19	11.39
Havana	23	8	82	28	3	6.31	6.84	6.81	7.25	5.41	5.82	4.92	4.77	5.51	6.04	7.05	6.70
Saint Thomas	18	20	64	56	1	5.04	6.68	3.34	3.22	5.44	6.24	6.06	4.49	4.07	3.16	2.08	4.84
Kingston......	17	58	76	47	2	3.84	3.67	3.76	4.47	1.09	6.42	6.11	3.12	4.05	3.52	2.75	3.17
Mean	....		....		......	7.12	7.66	7.55	7.94	7.47	7.46	7.17	5.79	5.98	6.70	6.76	7.04
Storms	....		....		......	.....	.....	.....	.....	.....	.....	.....	14.44	14.00	12.81	.....	.....
Ratio	....		....		......	.....	.....	.....	.....	.....	.....	.....	2.49	2.34	1.91	.....	.....

Column 4 shows the number of years of observations employed, and the velocities are expressed in miles per hour. The line marked *mean* shows for each month the average of the numbers for the several stations; the next line shows the rate of progress of storm centers as derived from Tables III and IV for the months August, September, and October. These are the only months for which the tables furnish more than a single observation, with the exception of June, for which month there are three observations. In determining the average rate of progress of storm centers I have rejected the velocity given for No. 4 of Table III, because it differs widely from all other velocities recorded in the two tables, and because it was derived from insufficient data. The last line of the table shows the ratio of the numbers in the two preceding lines.

95. I next endeavored to determine the mean velocity of the wind for that part of the Atlantic Ocean in the neighborhood of the usual tracks of storm centers, and have adopted the results contained in No. 3 of the Mittheilungen aus der Norddeutsche Seewarte, as exhibited in the pamphlet No. 5 (non-official), published by the British meteorological committee. The first line of the following table presents a summary of these results for the four seasons of the year, the force of the wind being estimated in units of Beaufort's scale (1–12).

	Winter.	Spring.	Summer.	Autumn.
Beaufort's numbers	5.9	5.5	4.5	5.3
Miles per hour	33.0	30.8	25.5	29.7
Storms	18.4	18.6	16.5	18.6
Ratio	0.56	0.60	0.65	0.63

The second line shows the velocities denoted by Beaufort's numbers reduced to miles per hour according to the table prepared by the British meteorological committee; the third line shows the average rate of progress of storms according to article 89, and the fourth line shows the ratio of the numbers in the two preceding lines.

96. If we group together the results now obtained we shall have the following summary for the average rate of progress of storm centers, the average velocity of the winds, and the ratio of these two velocities.

	Storms.	Winds.	Ratio.
United States	28.4	9.5	3.0
North Atlantic Ocean	18.0	29.8	0.6
Europe	16.7	10.3	1.6
West Indies	13.7	6.2	2.2
Southern Asia	8.4	6.5	1.3

This table appears at first view to present a discouraging medley of anomalies, but some of the anomalies may appear less formidable after a careful examination. It seems highly probable that the slow progress of storm areas in Southern Asia is partly due to the small velocity of the winds of that region. It is not obvious why storms should travel more rapidly near the West India Islands than in the China Sea. It is possible that this anomaly may disappear when the mean velocity of the wind has been determined by a more extensive series of observations.

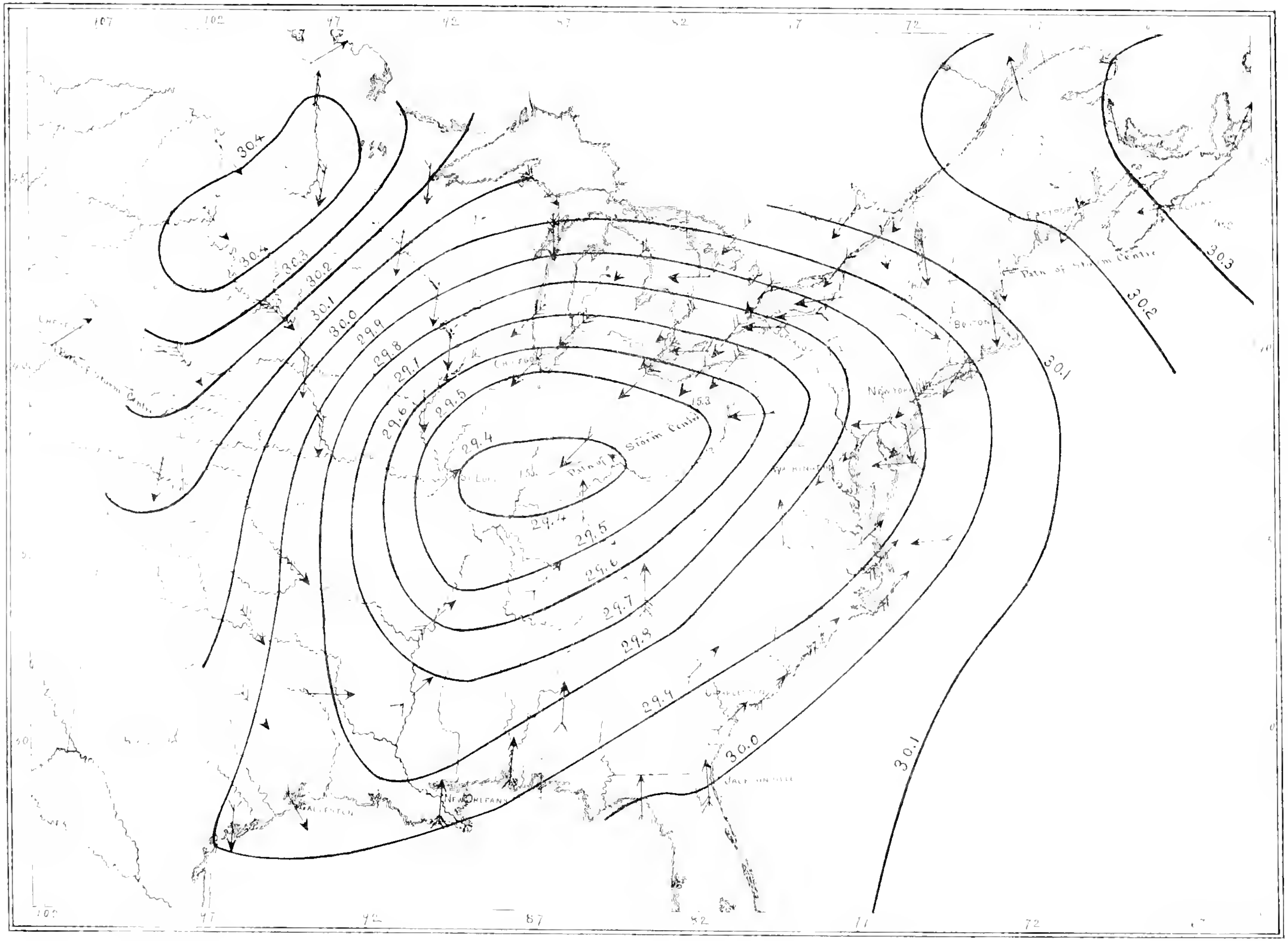
It seems to be established that over the North Atlantic Ocean the mean velocity of the wind is considerably greater than the rate of progress of storms. This inequality is strikingly exhibited in numerous cases. Over this ocean we frequently find an area of low pressure 2,000 miles or more in diameter, with a pressure of about 28 inches at the center, attended by winds blowing with hurricane violence (see Nos. 14, 29, 32, 41, and 44 of Table XXXV), while from day to day the center of the low area makes little or no progress eastward, showing that the movement of the atmosphere which corresponds to the average system of circulation is almost entirely interrupted over this ocean.

The most noticeable anomaly shown in the preceding table is, however, presented by the United States, where the mean velocity of the wind is only one-third as great as over the Atlantic

Ocean, but storms travel with nearly double velocity. This anomaly may be partly explained if we admit that the progress of storms is determined, not by the wind which prevails in close contact with the earth's surface, but by that which prevails at an elevation of several hundred feet, where the velocity is probably much greater than at the earth's surface. The same anomaly, however, is found when we compare the storms of the United States with those of Europe. In Northern Europe the surface winds have a velocity greater than those of the United States, and we may infer that the same is true for elevations of 1,000 or 2,000 feet above the surface, yet storms in Europe advance with but little more than half the velocity of those in the United States. There must then be a powerful cause which accelerates the movement of storm areas in the United States, and which does not operate in Europe or over the Atlantic Ocean; and apparently the same cause does not operate in Southern Asia or in the West Indies, at least in an equal degree. This cause (or one of these causes) is probably the precipitation in the form of rain or snow which usually takes place on the east side of a storm area, greatly in excess of that on the west side, by which means the progress of the storm center is greatly accelerated. This is a question which will be carefully examined hereafter.

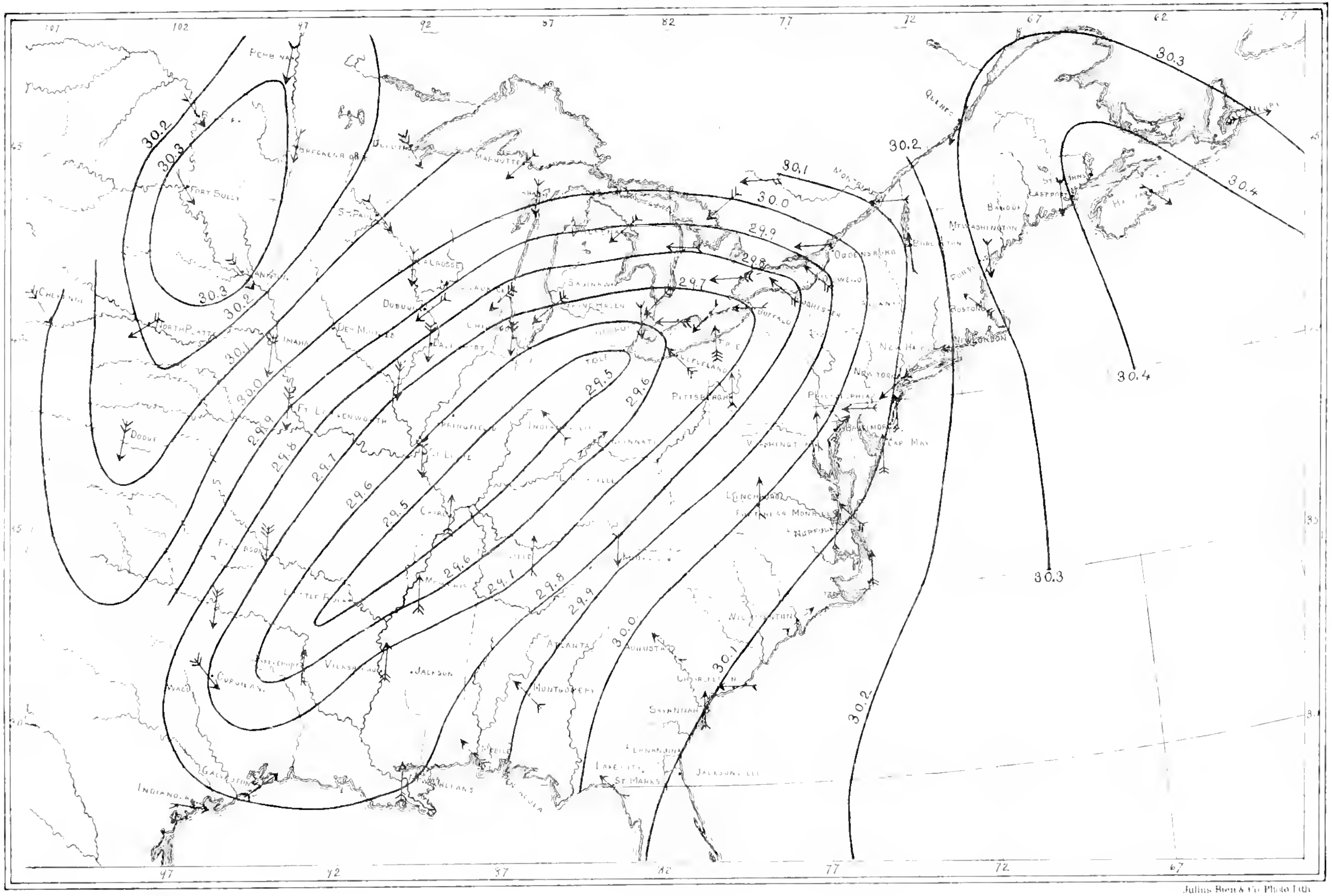
STORM OF JANUARY 15 1877

PLATE I



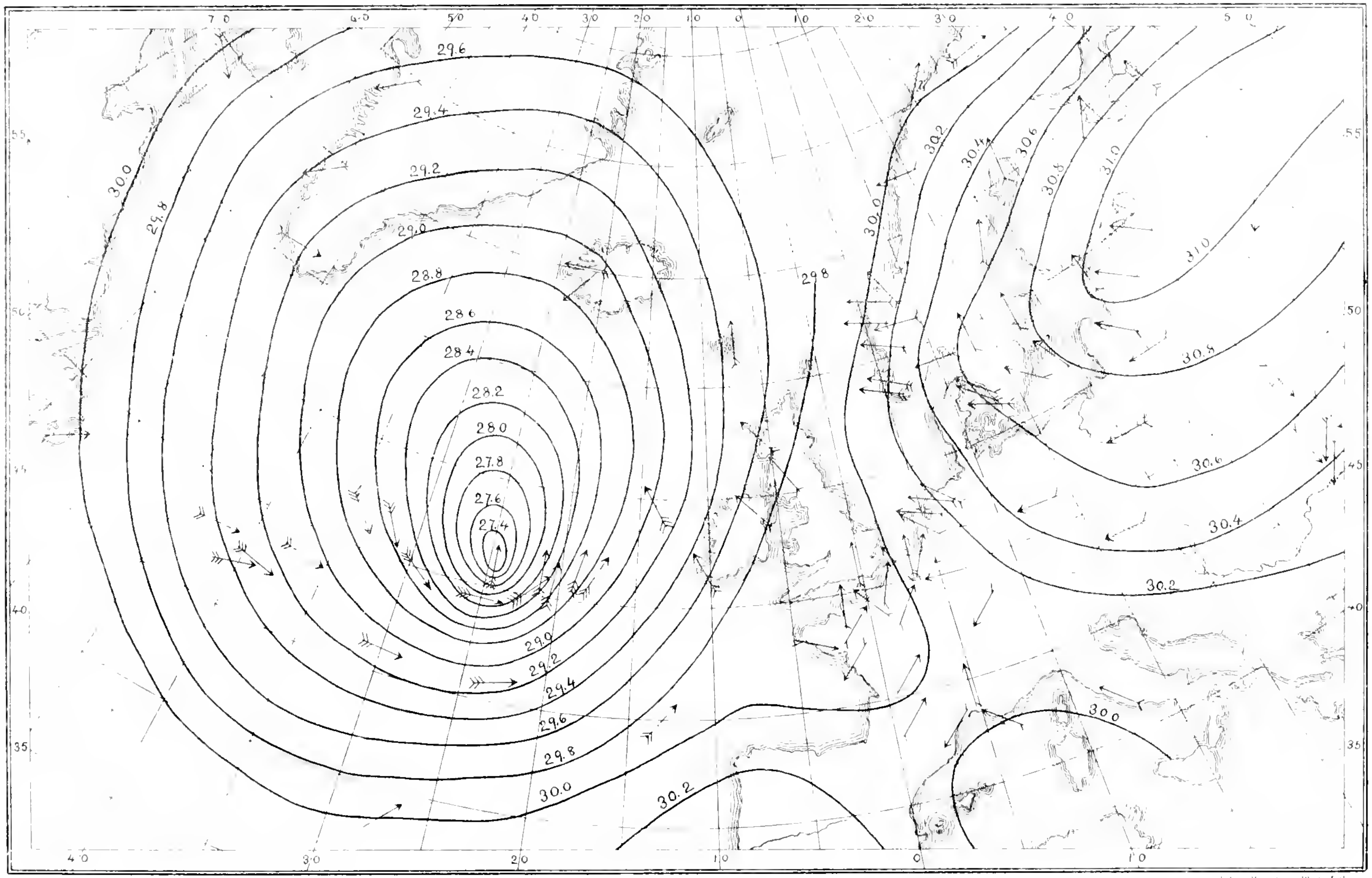
STORM OF MARCH 8. 1877

PLATE II.



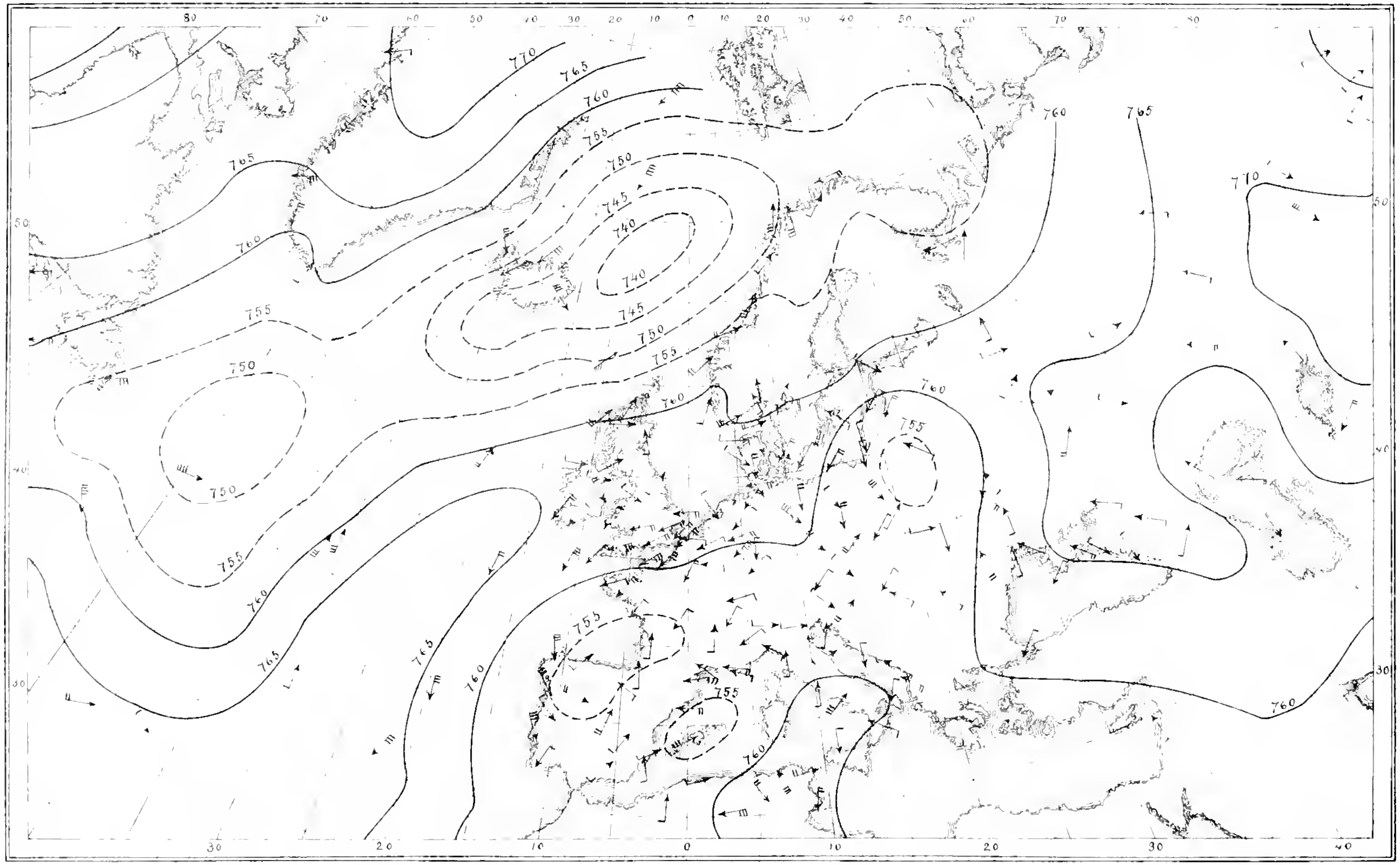
Julius Bien & Co. Photo Lith.

STORM OF FEBRUARY 5 1870. PLATE III

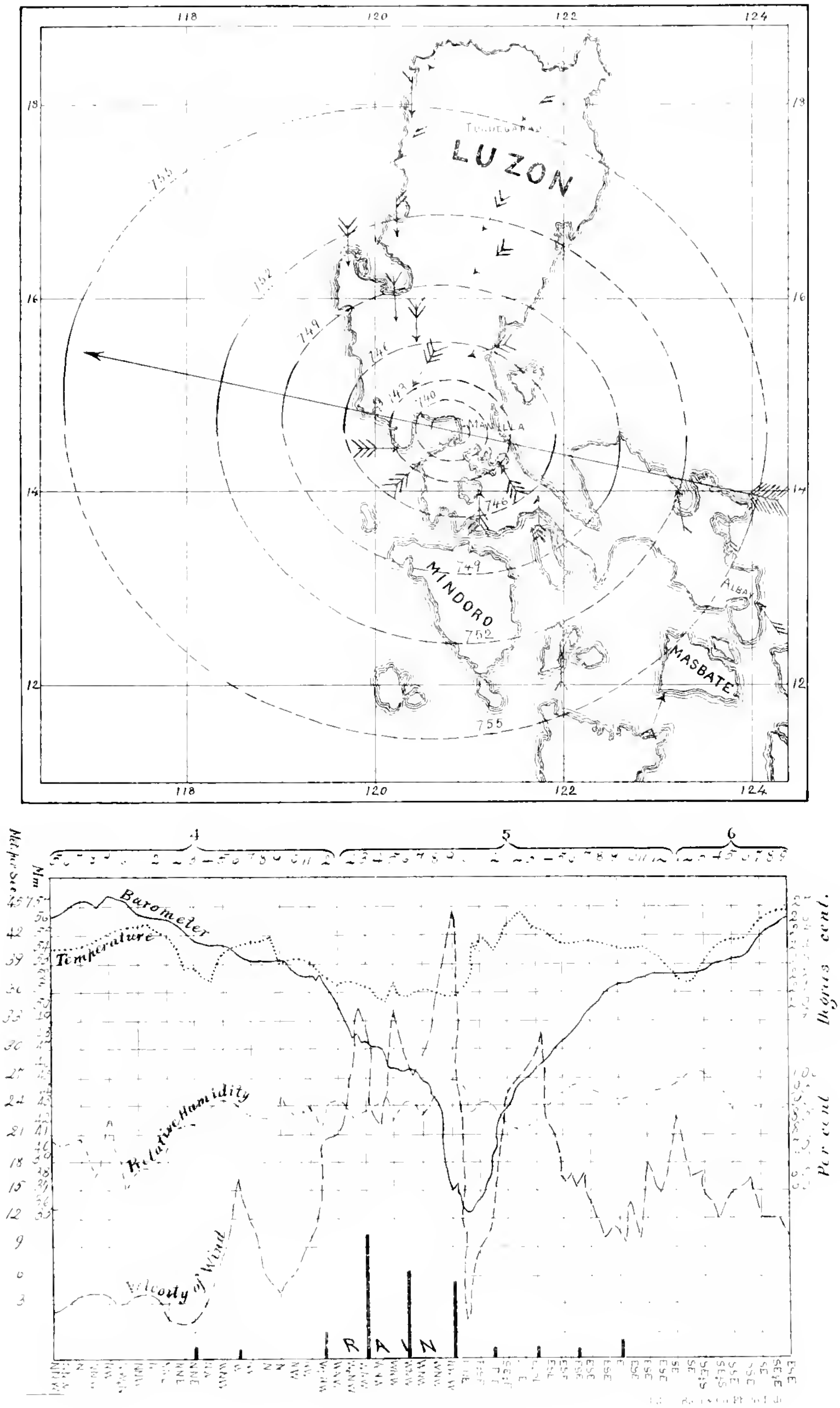


STORM OF MAY 31 1875

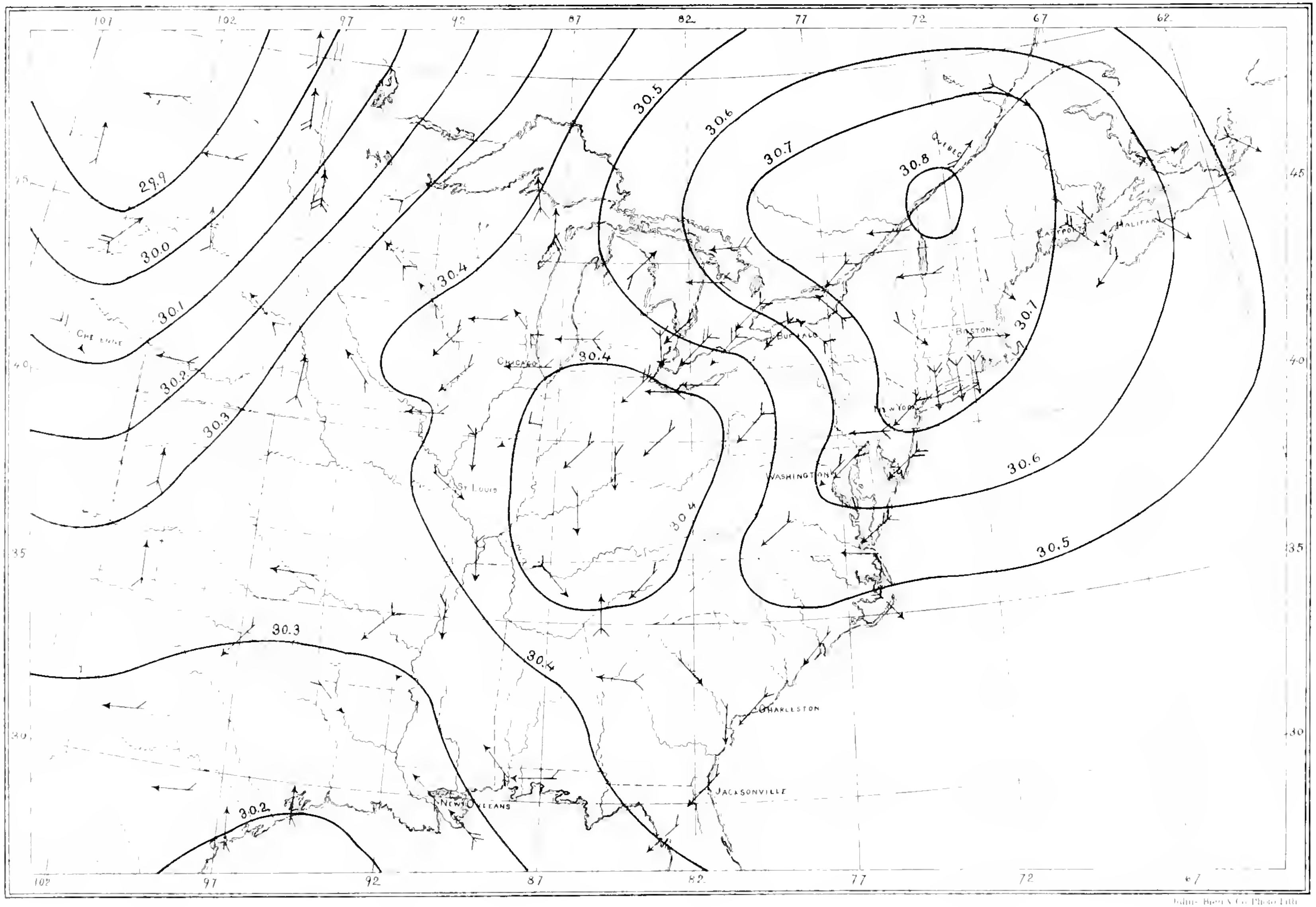
PLATE IV.



MANILLA TYPHOON NOV 5. 1882 PLATE V

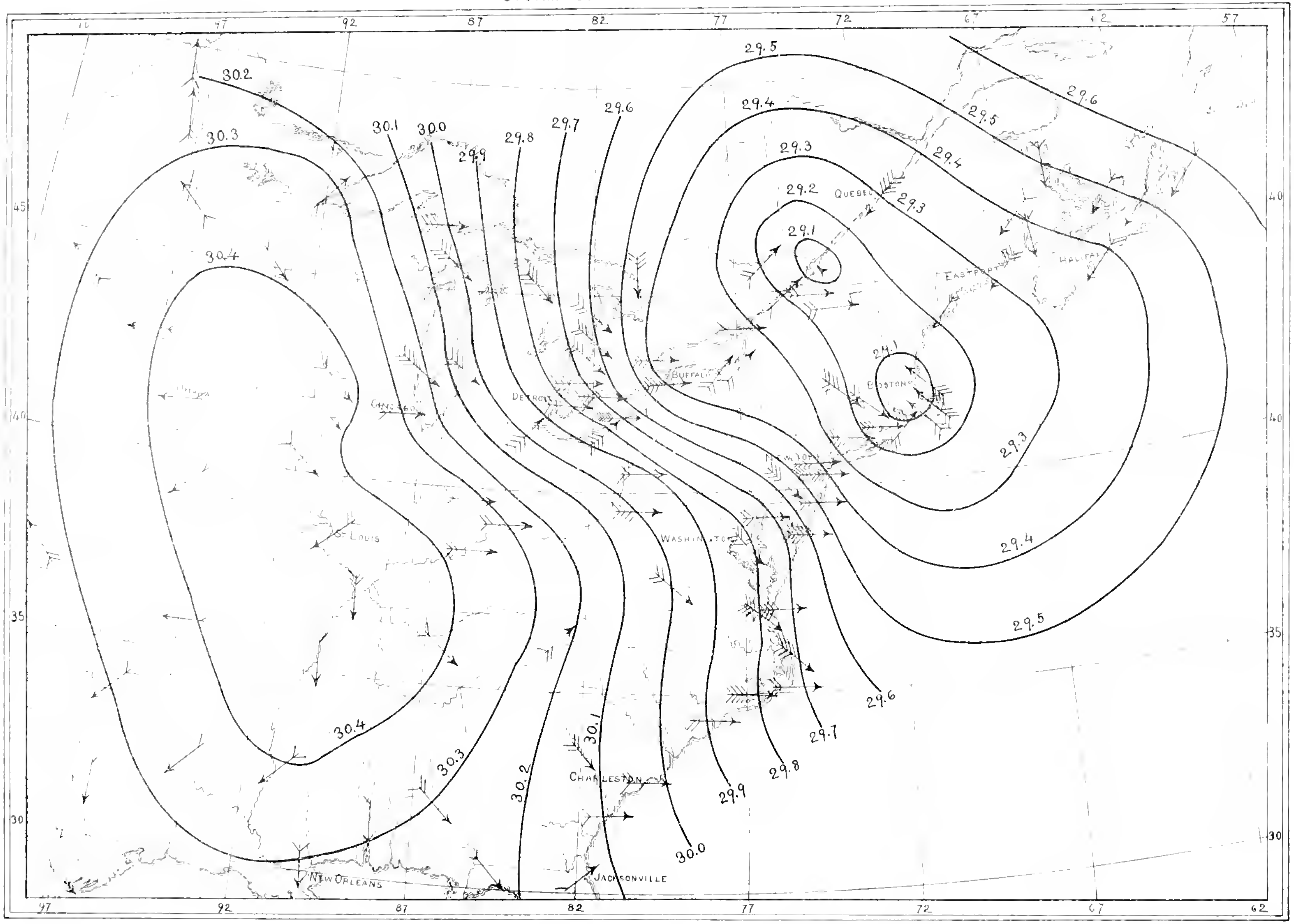


ISOBARS JANUARY 5 1882. PLATE VI

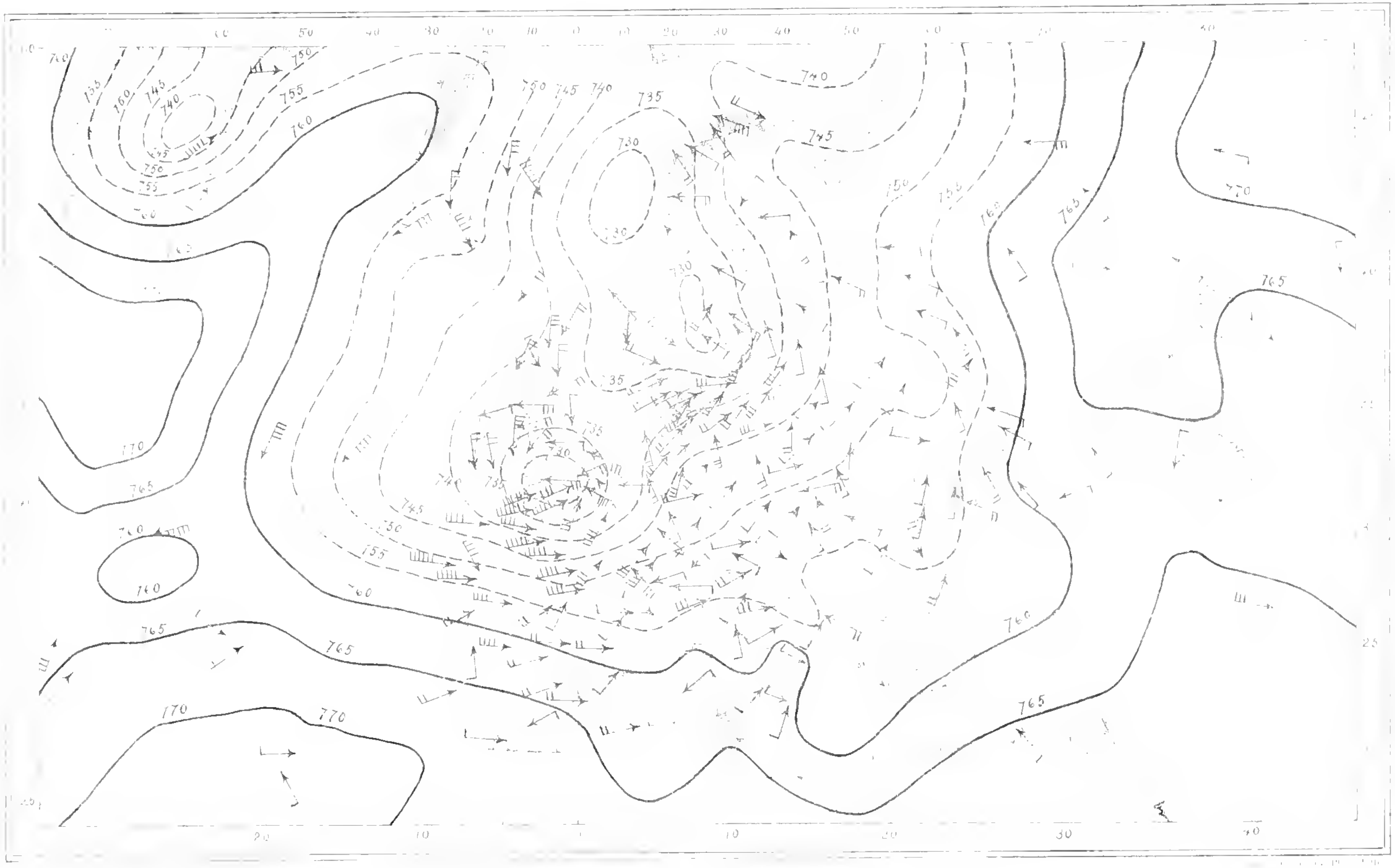


STORM OF DECEMBER 9. 1876

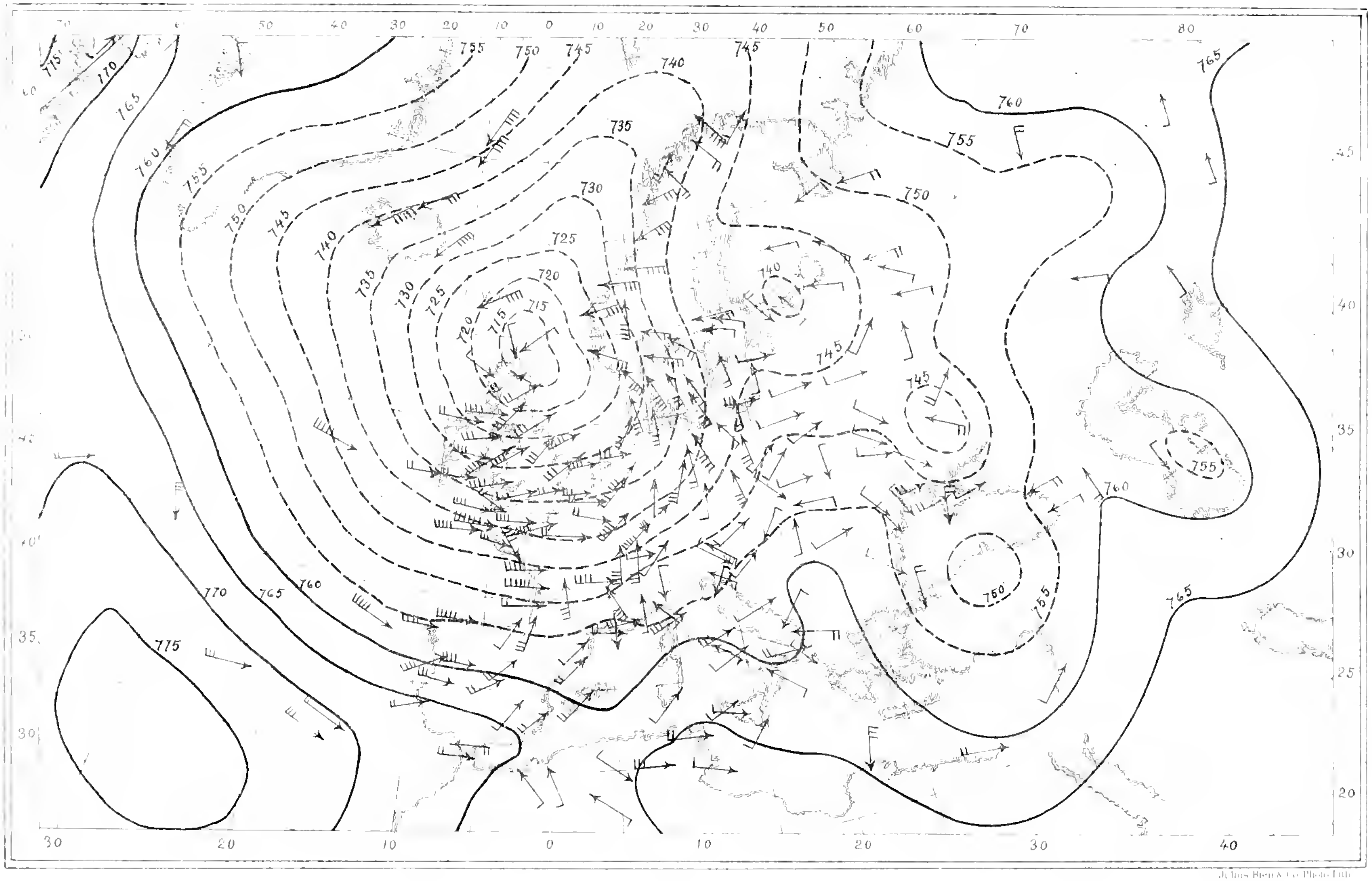
PLATE VII.



STORM OF MARCH 12 1876

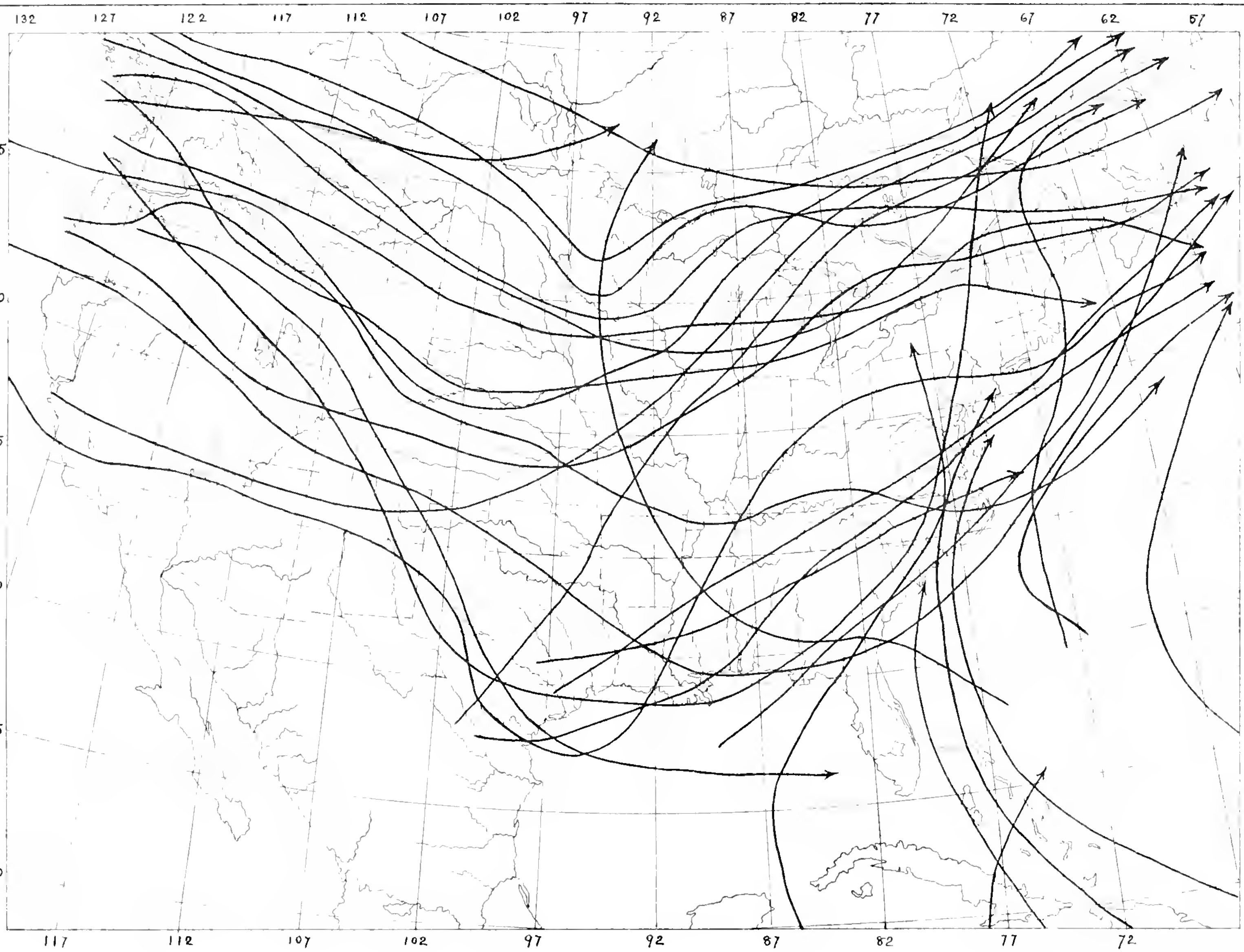


STORM OF MARCH 9. 1876. PLATE IX



STORM PATHS OF THE UNITED STATES

PLATE X.



Julius Bien & Co. Photo Lith.

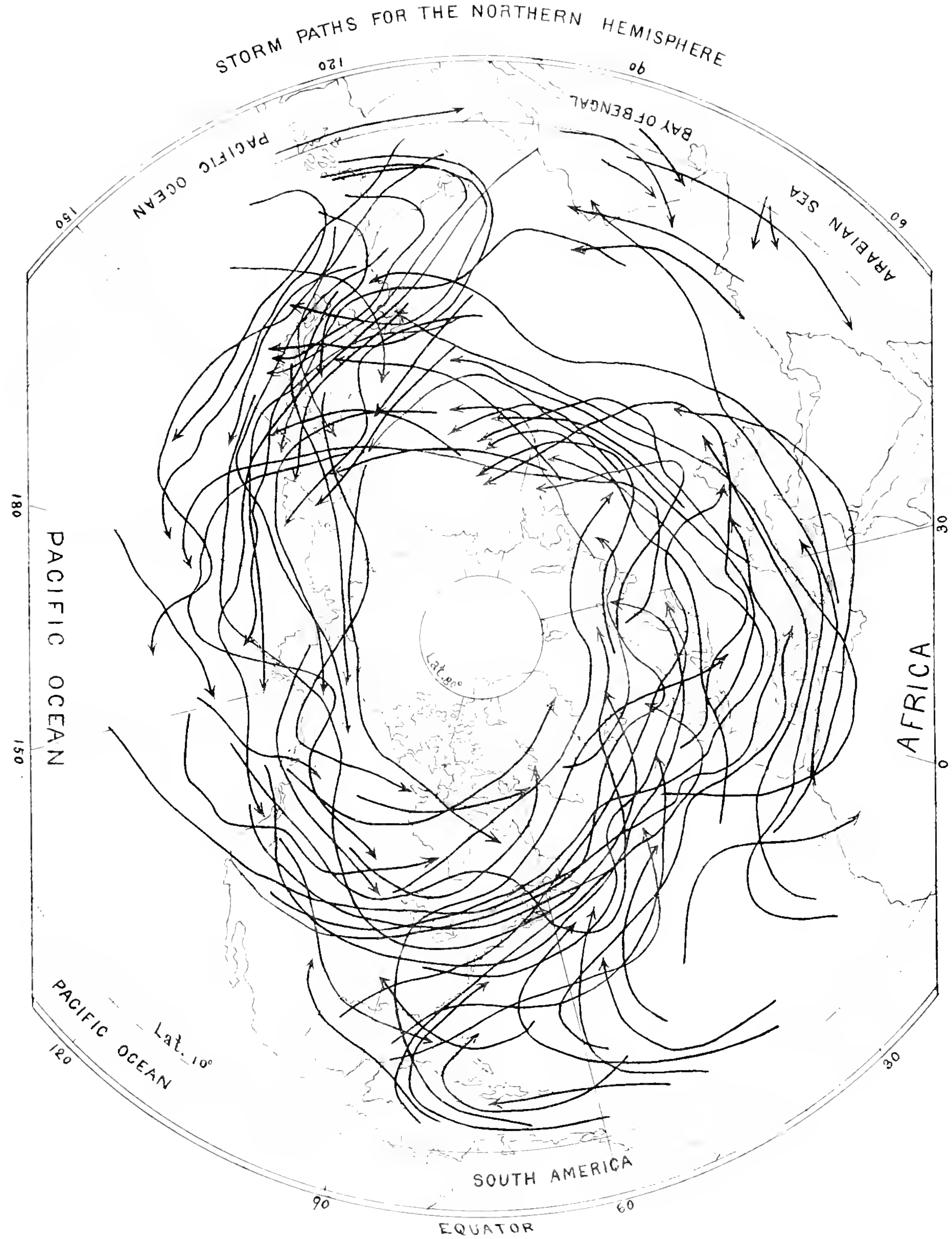

STORM PATHS FOR THE NORTHERN HEMISPHERE
120
90
150
60
PACIFIC OCEAN
BAY OF BENGAL
ARABIAN SEA
180
30
PACIFIC OCEAN
AFRICA
Lat. 80°
150
0
PACIFIC OCEAN
Lat. 10°
120
30
SOUTH AMERICA
90
60
EQUATOR

FIG. I

TRACKS OF AMERICAN STORMS ADVANCING WESTERLY

PLATE XII

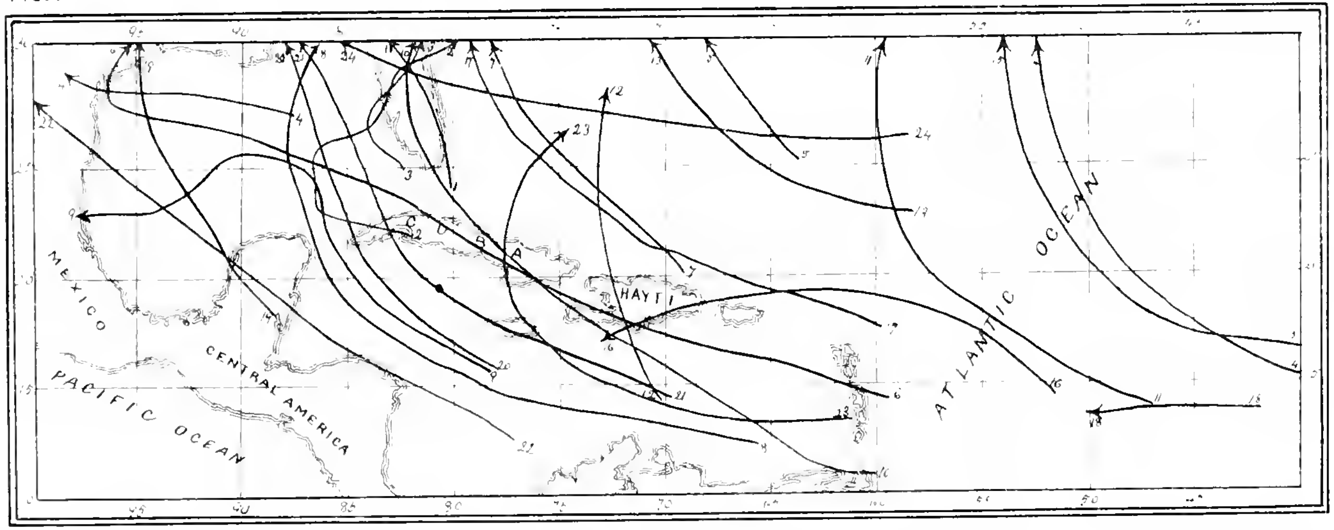
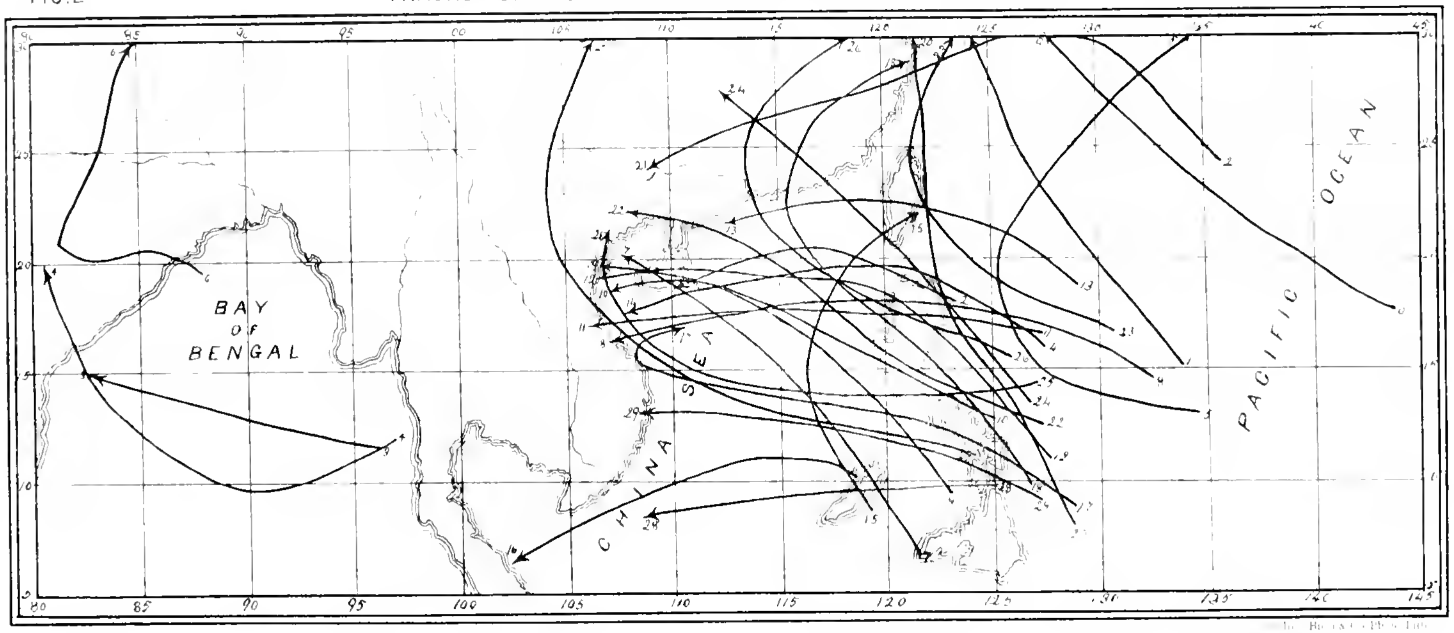
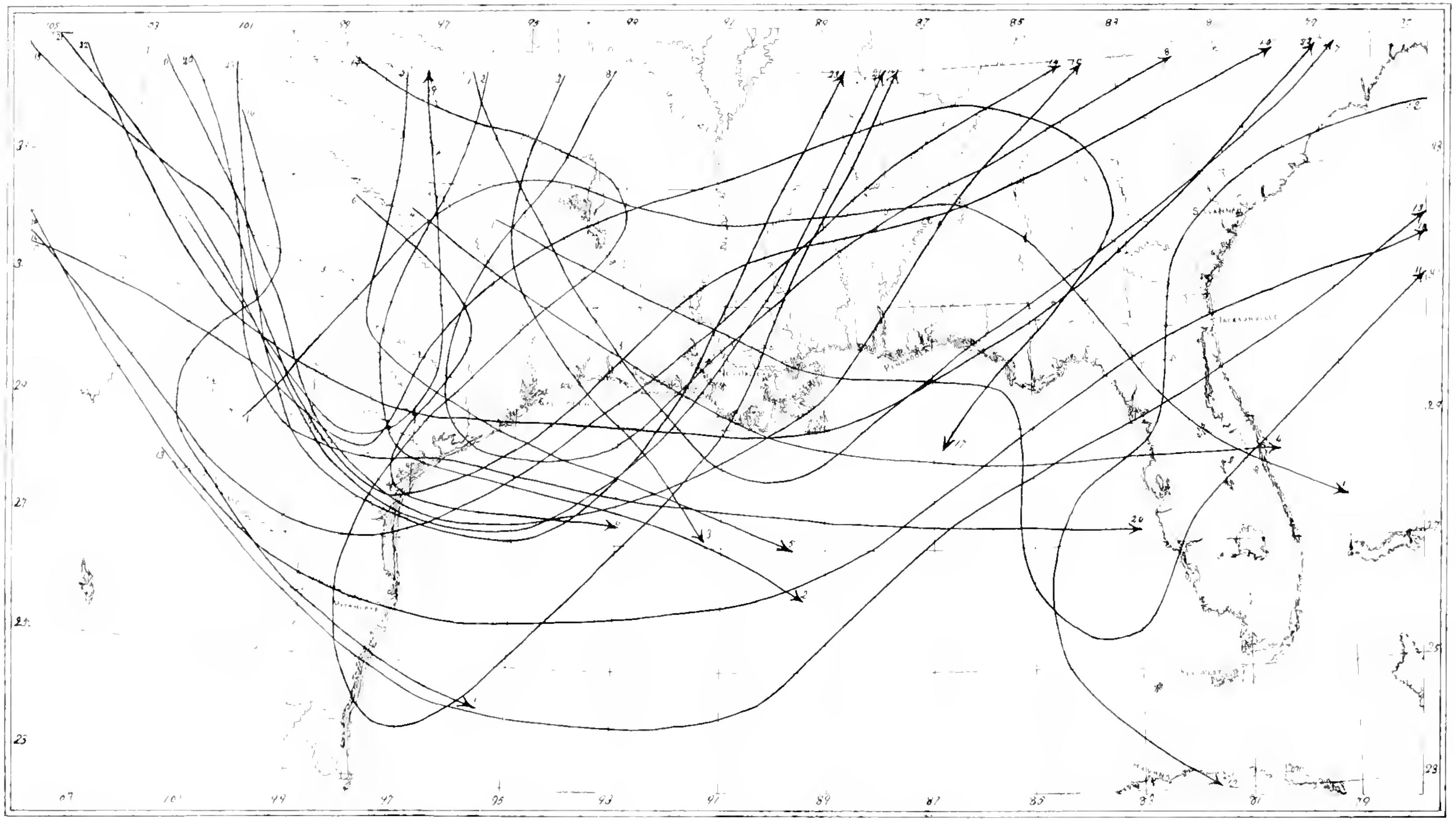


FIG. 2

TRACKS OF ASIATIC STORMS ADVANCING WESTERLY

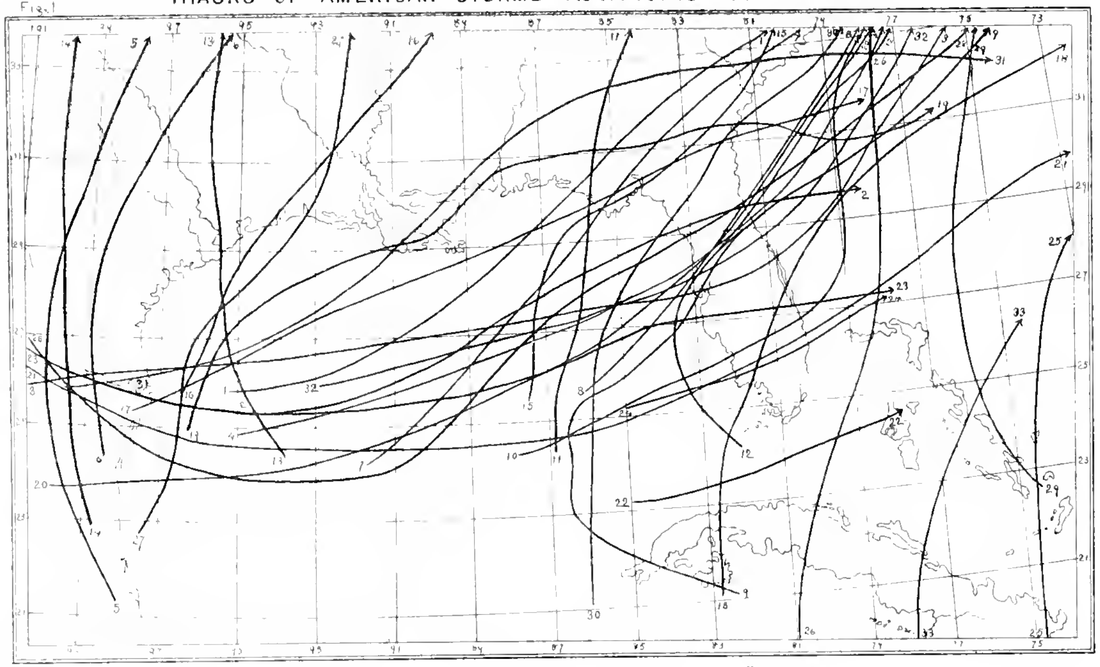


TRACKS OF AMERICAN STORMS ADVANCING SOUTH-EASTERLY



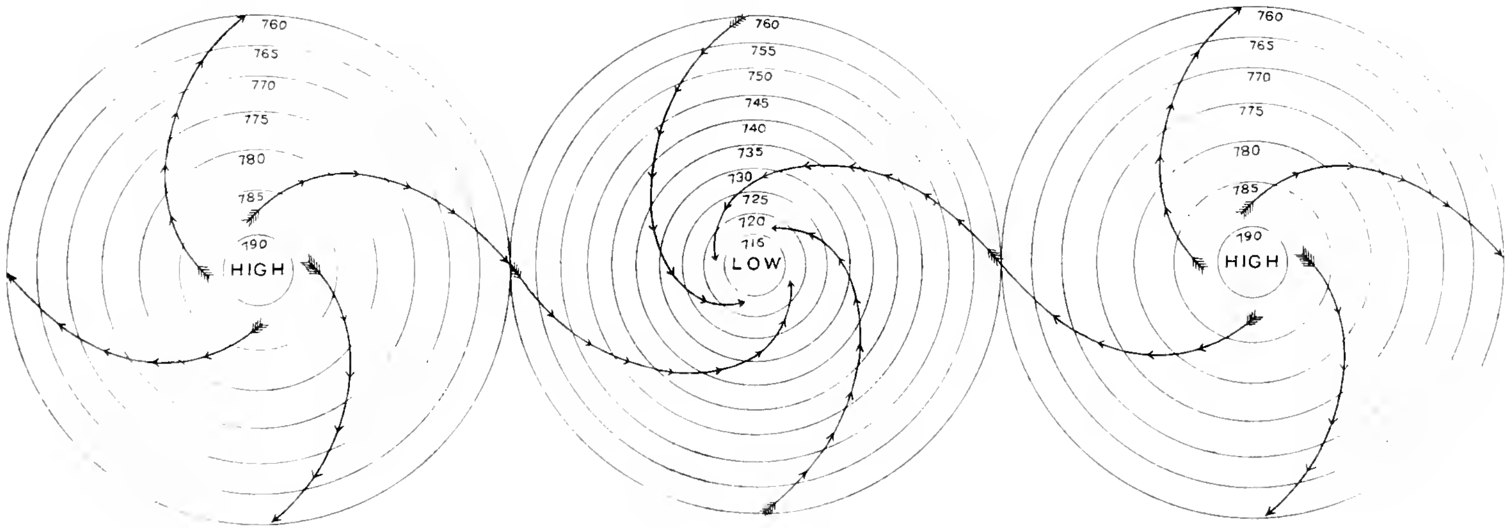
TRACKS OF AMERICAN STORMS ADVANCING NORTHEASTERLY PLATE XIV.

Fig. 1



SPIRAL MOVEMENT OF THE WIND

Fig. 2



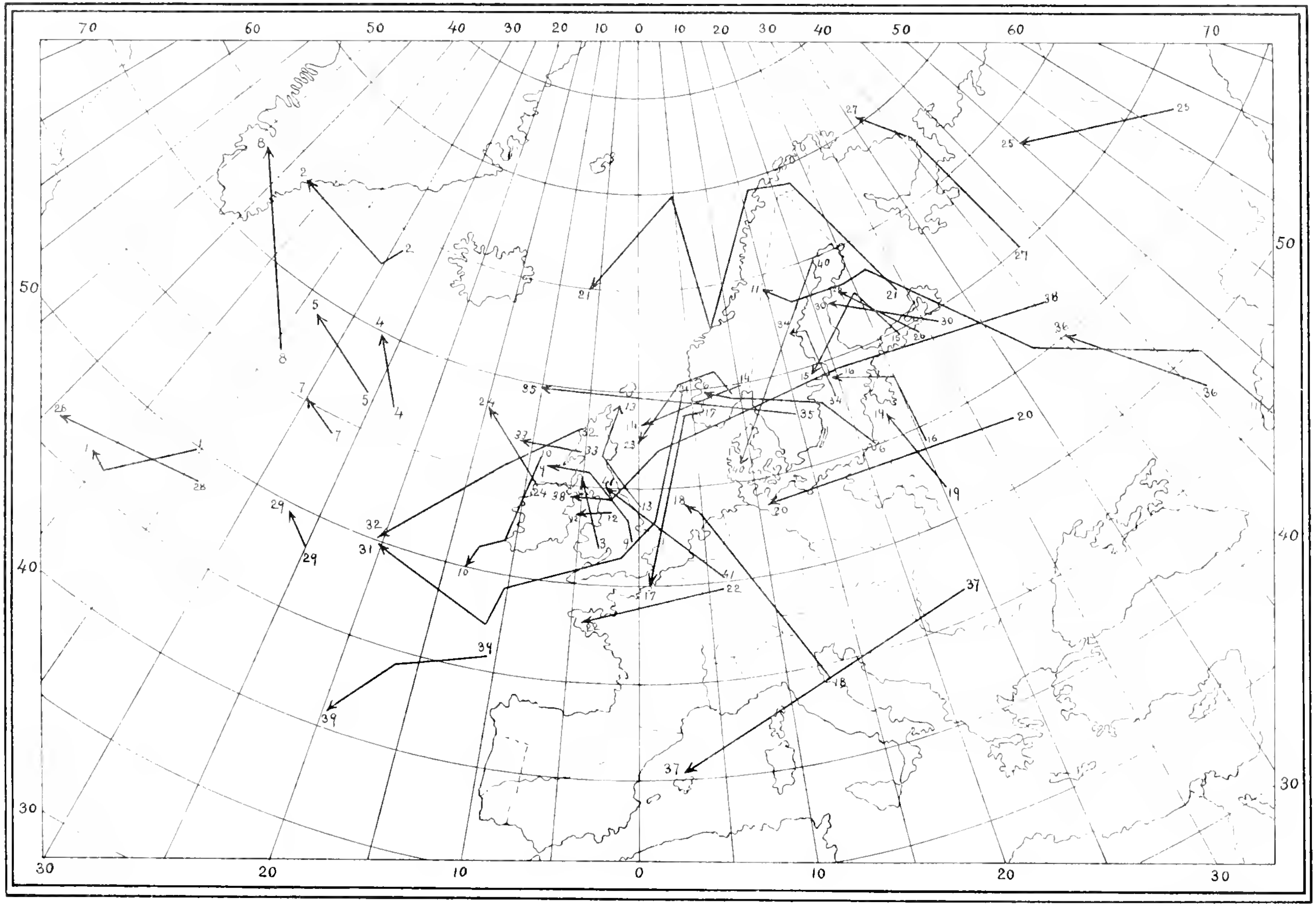
Julius Bien & Co. Photo Lith.

STORMS IN THE UNITED STATES ADVANCING WESTERLY

PLATE XV

STORMS OVER THE ATLANTIC OCEAN AND EUROPE ADVANCING WESTERLY

PLATE XVI



NATIONAL ACADEMY OF SCIENCES.

VOL. III.

TENTH MEMOIR.

ON FLAMSTEED'S STARS "OBSERVED, BUT NOT EXISTING."

FLAMSTEED'S STARS "OBSERVED, BUT NOT EXISTING."

READ NOVEMBER 11, 1885.

By C. H. F. Peters.

In the "Account of the Rev. John Flamsteed," by Francis Baily, on page 646, is found a list of stars with the above heading, which means, as is explained on page 393, stars "of which the observations appear to be accurately recorded, but which still cannot now be found in the heavens."

When, with the splendid and exciting discoveries of Sir W. Herschel, there had come life among the fixed stars, and the construction of the heavens began to be a field for active research as well as for speculation, it was but natural that, whenever in a place given by the Catalogus Britannicus no star was seen, at first thought the star was believed to have become extinguished since Flamsteed's time. Under this impression, it seems, various astronomers, and among them especially Lalande, then engaged upon his zone survey, from time to time published long lists of "missing" stars. Already Bode, however, curtailed these considerably, and the number of such stars gradually has been diminished, thanks to the labors especially of Miss Caroline Herschel, systematically indexing and comparing with the British Catalogue all the observations contained in the second volume of the Historia Cœlestis Britannica;—then of Argelander and of Baily, so that the latter finally leaves only 22 stars to be accounted for. It might seem fruitless to attempt a revision of positions that have passed through the hands of such able critics and been dismissed by them as inexplicable, especially as Baily made a very thorough inspection of Flamsteed's original manuscript entries, preserved at the Royal Observatory, Greenwich. Nevertheless, as the disappearance from the skies of so many stars in comparatively so short an interval of time is rather improbable, it seems desirable that these cases be scrutinized somewhat more thoroughly than perhaps it was feasible for Baily, who, having taken in hand the revision of the catalogue in its entirety, could not well devote so much time to a few particular stars. The resources, besides, for the sake of identification, are much more complete now than they were at the time of Baily's publication, dating back fifty years. Flamsteed did not observe stars fainter than the eighth magnitude.* Hence all of his stars between the pole and 92° north polar distance, if not belonging to the classes of variable and temporary stars, we must expect to find in the "Durchmusterung." Con-

* In the British Catalogue the magnitude 8 is found assigned to 24 stars. According to modern catalogues all of them are brighter, with the exception of the following 3, viz:

B. Fl. 1223	in the Bonn Dm.	$8^m.3$
1280		8 .2
1613		8 .0

Besides there are 23 stars without having any magnitude assigned to them in the British Catalogue. Of these, according to modern observers, the following 5 are smaller than eighth magnitude:

B. Fl. 792	in the Dm.	$8^m.2$
1418		8 .1
1422		8 .2
3256		8 .2
3257		8 .2

while the remaining 18 are estimated brighter.

cerning the positions of the more southern stars that come under consideration in the following discussion, I have consulted the skies directly.

It is well to premise here, that of the 22 stars that Baily left unidentified, Flamsteed gave to 3 the magnitude of 5, to 1 the magnitude of $5\frac{1}{2}$, to 9 the magnitude of 6, to 1 the magnitude of $6\frac{1}{2}$, to 3 the magnitude of 7, and of 5 (2 of which belong to the group of ***Præsepe***) the magnitudes are not recorded.

Fortunately, what discrepancies Baily detected in the manuscripts are communicated in the copious notes to the new edition of the British Catalogue, so that for the stars in question we may take the figures as they are given for time and zenit distance in Volume II of the Historia Cœlestis for the actually recorded ones, without any apprehension of errors of print.

For none of these stars is there more than one observation, and the record of it may involve a mistake, either in noting the clock time, or in circle reading, or in hearing and writing by the amanuensis. For the reading of the circle "*per lineas diagonales*" a check is given by that "*per strias cochleae.*" This, however, may not always hold good, as, for example, there are indications that sometimes (perhaps when the stars came in quick succession, or for some other reason) only the fraction was read off or put down for the screw, and the whole number of revolutions afterwards filled in from the corresponding reading of the division.

When, by some hypothesis of a mistake, we try to bring about an agreement with a star known by modern determinations, the question naturally presents itself, What difference are we allowed to admit? or, in other words, since the modern observations in this comparison may be assumed as perfectly accurate, what is the probable error of a position in Flamsteed's Catalogue? For obtaining approximately an idea of this, may serve the differences from Bradley, which Baily has added in his edition. By taking from twenty pages (about every fifth page) the mean of these differences, without regard to sign, excluding, however, the stars of high northern declination, also avoiding those with a known considerable proper motion and those which clearly appear to be affected with some gross accidental mistake, I find 44″ in right ascension and 17″ in declination. By another count, viz., by taking the means of differences for all the sixth-magnitude stars that were observed only once (about 400 in number), I find 50″ and 18″, respectively. Disregarding as small what the comparison with Bradley may have added, these figures give an approximate measure of the mean uncertainty of a position in the British Catalogue. Considerably much smaller is the pure mean observation error of Flamsteed, which Argelander has computed, viz., $\pm 10''.4$ in right ascension and $\pm 7''.8$ in declination.* These values are independent of the situation of the Quadrant, while the others are affected, besides, by the imperfection of the elements Flamsteed used in his computations. The contrast is a proof how greatly the usefulness of the Catalogus Britannicus could be increased by a re-reduction of the original observations.

Proceeding now to the examination of each of the 22 stars in particular, it seemed necessary to communicate the discussion with some detail, in order to leave as little doubt as the subject in each case permits. Every one accustomed to observe is aware of the facility of committing mistakes, however careful he may believe he has been. The assumption, therefore, of some otherwise plausible error of Flamsteed which leads to a modern star-place is to be held much more reasonable than the vague acquiescence in a supposed disappearance of the star. In that sense I think I have succeeded in finding for every case at least a probable explanation.

1) B. Fl. 314.

This star in the British Catalogue is called 28 *Arietis*, and its position given thus:

$$6^{\text{mag}}\ 33^\circ\ 33'\ 20''; \ +18^\circ\ 28'\ 40''$$

which reduced to 1800 would be

$$35^\circ\ 4'\ 44''; \ +18^\circ\ 59'\ 3''$$

* Argelander (De observationibus astron. a Flamsteedio institutis dissertatio. Regiomont., 1822) finds the *probable* error of a pointing in right ascension $\pm 0^s.3263$, or of the right ascension itself, being the result of the differentiation from a so-called "determining star," $\pm 0^s.3263\ \sqrt{2} = \pm 0^s.4614 = \pm 6''.9$, of the zenit distance or declination $\pm 5''.20$. Hence follow the *mean* errors as given in the text.

where there is no star to be seen now. But Pi. 2^h. 98, 26 *Arietis* (Dm. +19°.365 . 6^m.4) differs about 15′ in right ascension. In fact, we have

26 Arietis for 1800:	34° 51′ 44″;	+18° 57′ 36″
Precession to 1690	−1 31 22	−30 28
Proper motion in 110^y (Mädl. Br.)	−7	+5
26 Arietis for 1690:	33 20 15;	+18 27 13
26 Arietis — Fl. 314	−13 5;	−1 27

By reading, therefore, in the Historia Cœlestis Britannica, 1692, December 10, the time of transit, 8^h 18^m 58^s instead of 8^h 19^m 58^s, we come upon this star, and the star denominated 28 *Arietis* has originated from a mistake of 1^m. Already Miss Herschel had made this conjecture, and Baily had hardly any reason not to accept it.

2) B. Fl. 639.

The absence in the skies of the star, which in the British Catalogue is reported thus:

100 Tauri 6^{mag} 70° 2′ 0″; 15° 50′ 5″

seems to have puzzled much the older astronomers, and Bode, assuming the observation to be unmistakable (*Astronomisches Jahrbuch*, 1788, p. 175), takes pains to show (*ib.*, 1817, p. 249) that it could not have been any one of the four asteroids that were then known.

In the Historia Cœlestis, II, p. 389, stand the following observations:

1700.	Clock time.	Zenit dist. lineæ diagonales.	Striæ cochleæ.	Corrected zenit distance.	
	h. m. s.	° ′ ″		° ′ ″	
Jan. 1 ☽	8 58 9	35 44 45	810.21	35 36 5	α Tauri, *Palilicium.*
	9 11 59	36 11 20	820.20	36 2 40	
	9 20 5	35 45 25	810.48	35 36 45	
	9 21 55	36 2 50	817.06	35 54 10	
	....	36 39::50	831.13	36 31 10	
Index error..	....	−8 40			

Baily found this in accordance with the MS., except that at the third star (which is exactly the questionable one) "the zenith distance is marked as doubtful in the original MS. entry." (The sign of uncertainty (::) attached to the last star perhaps was placed two lines too low in the print?) The other stars are identified by means of α Tauri. Reducing the times of transit for rate of clock to 9^h 20^m, the third and fourth columns of the next table are obtained.

B. Fl.	Name.		Reduction to sidereal interval.	T=clock time reduced for rate.	Striæ cochleæ in arc.	Difference.
			s.	h. m. s.	° ′ ″	″
581	87	α Tauri	−3.6	8 58 5	35 45 6	−21
611	96	Tauri	−1.3	9 11 58	36 11 27	− 7[−21]
639	(100)?	Tauri	0.0	9 20 5	35 45 11	+14
642	101	Tauri	+0.3	9 21 55	36 3 9	−19[−15]
652	11	Orionis	....	....	36 40 17	−27

In the same table are added, in column 6, the *striæ cochleæ* readings converted into arc, and in column 7 the differences of these with the readings *per lineas diagonales*, which show at least that both indications are in pretty good harmony with each other. With greater regularity, however,

proceed the figures in this column if we adopt the corrections presently to be spoken of, and which leave the differences added in brackets.

For the known stars modern determinations furnish the following positions, referred to the equinox of 1700:

Name.	No. and magnitude of Durchm.		α	δ
	°	m.	h. m. s.	° ′ ″
87 α Tauri	+16.629	1.1	4 18 45	+15 52 2
96 Tauri	15.687	6.5	4 32 37	15 20 25
101 Tauri	15.713	7.0	4 42 33	15 25 30
11 Orionis	15.732	5.3	4 47 27	14 56 44

The difference α—T should be constant. Further, the observed zenit distance (*per lineas diagonales*, which Flamsteed always uses, in preference to that *per strias cochleæ*) should result when subtracting the declination from 51° 36′ 38″, which is the sum of 51° 28′ 38″+8′ 40″—40″ (latitude of Greenwich+index error—refraction). We get—

Name.	α—T	Computed zenit dist.	Comp.—obs. zenit dist.	The same, with assumed correction.
	h. m. s.	° ′ ″	′ ″	″
87 α Tauri	—4 39 20	35 44 36	— 9	— 9
96 Tauri	4 39 21	36 16 13	+ 4 53	— 7
101 Tauri	4 39 22	36 11 8	+ 8 18	+ 18
11 Orionis	....	36 39 54	+ 4	+ 4
Mean	—4 39 21			+ 1

The large errors remaining here, column 4, in the declinations of 96 and 101 *Tauri* have been pointed out already by Baily (in Notes to No. 611 and No. 642 of British Catalogue), without the attempt of a conjecture as to their origin, the MS. entry showing no trace of mistake. I venture to suggest for the correct zenit distance record:

96 Tauri 36° 16′ 20″ 822.20 instead of 36° 11′ 20″ 820.20
101 Tauri 36° 10′ 50″ 820.06 instead of 36° 2′ 50″ 817.06.

This rests upon the hypothesis, that of the *striæ cochleæ* first were written only the fractions, and the whole revolutions supplemented later so as to correspond with the respectively 5′ and 8′ erroneous *lineæ diagonales*. At least in the case of 96 *Tauri* the changes proposed are highly probable. Converted into arc the screw readings are then

822.20=36° 16′ 44″ and 820.06=36° 11′ 5″,

leaving the differences from the *lineæ diagonales* of —24″ and —15″ respectively (the values in brackets in the table above), in better agreement with the two other stars, and also, as seen from the last table, in good harmony with the computed zenit distances.

Now, as to the disputed star, it must be between 96 and 101 *Tauri*. Here the following stars are the only ones of sufficient brightness for possible objects of Flamsteed's observations:

Number and magnitude of Dm.		Piazzi.		Right ascension (1700).	Declination (1700).
°	m.	h.	m.	° ′ ″ h. m. s.	° ′ ″
+16.667	7.5	4.228	8	69 25 48=4 37 43	+15 51 34
16.668	7.0	4.231	8	69 31 32=4 38 6	16 5 47
16.672	5.6	4.246	6.7	70 1 18=4 40 5	16 38 34

The last star is Br. 686, wherefore its position was taken from Mädler's Bradley, while the others are from Piazzi's Catalogue. If we add to the right ascensions the constant $4^h\ 39^m\ 21^s$, and subtract the declinations from $51°\ 36'\ 38''$, we should get the clock time of transit and the apparent zenit distance (*per lineas diagonales*) that would have been observed by Flamsteed, viz:

	T	Z.	Differences.	
	h. m. s.	° ′ ″	m. s.	′ ″
Pi. 4^h 228	9 17 4	35 45 4	—3 1	— 21
Pi. 4^h 231	9 17 27	35 30 51	—2 38	—11 31
Br. 686	9 19 26	34 58 4	— 39	—47 21
Flamsteed's record is	9 20 5	35 45 25		

The zenit distance, within the probable error, is that of Piazzi 228, and by the assumption of an error of 3^m in the time we can make also the right ascension agree with that star. This seems to be, indeed, the most plausible explanation. There remains, however, one difficulty that speaks against this identification. Flamsteed noted the star he observed, of 6th magnitude, while Piazzi 228, is according to the Dm. $7^m.5$, and according to Piazzi and Bessel (W_2 4^h 1048) even only 8^m. There is here in the neighborhood the bright star Br. 686, which must have been in the field almost together with the spurious star, Flamsteed's telescope having a field of about $1\frac{1}{2}°$. It would be singular, indeed, if Flamsteed should not have observed this star at all. But by two plausible changes the record can be reconciled with it.

First, as to the time of transit. Suppose the observer (Flamsteed) called out "twenty-five," meaning 25 seconds, but the amanuensis wrote $20^m\ 5^s$, not inquiring, of course, further for the minute, which was 19. Hence, out of $9^h\ 19^m\ 25^s$ became $9^h\ 20^m\ 5^s$. The zenit distance can be corrected by the assumption of $34°\ 55'$ instead of $35°\ 45'$, a mistake in writing that is easily made and of which there are other examples in Flamsteed's manuscripts. Moreover, this observation was hastened by the next star following soon after. It remains to be seen how the check reading *per strias cochleæ* would have been. Leaving the fraction the same, but changing (upon the same ground as stated before in discussing the data for 96 and 101 *Tauri*) the number of entire revolutions, if we replace 810.48 by 791.48, the difference between the two modes of reading comes nearer to that derived from the other stars. For we have—

	Zenit distance per line as diagonales.	Zenit distance per strias cochleæ.	Difference (lineæ diagonales—str.)
	° ′ ″	° ′ ″	″
The record as it stands ..	35 45 25	810.48=35 45 11	+11
The record as corrected ..	34 55 25	791.48=34 55 39	—14

while the mean difference from the other stars (s. last column in second table above) is $-18''$ [or $-22''$ respectively].

The final position for 1700.0, thus corrected, becomes:

	Right ascension.		Declination.
	h. m. s.	° ′ ″	° ′ ″
Fl. observed	4 40 4	=70 1 0	+16 41 13
Br. 686 computed ..	4 40 5.2	=70 1 18	+16 38 34
Difference		+18	— 2 39

The error still remaining in declination appears rather large; but it will be remembered that Baily found in the manuscript the sign of uncertainty attached to the zenit distance.

3) B. Fl. 756 ($5\frac{1}{2}^{m}$).

As there is no $5\frac{1}{2}$ magnitude star in the position of the one, which in the British Catalogue is called 27 *Camelopardalis*, Argelander thought the observation of it, made on 1696, January 22, might be only a repetition of 24 *Camelopardalis*, still in the field.

The observations as they stand in the Historia Cœlestis, II, page 286, are shown in the first five columns of the following table. The other columns give the names of the stars, the arcs corresponding to the readings *per strias cochleæ*, and the differences of these with the readings *per lineas diagonales*.

Clock time.		Zenit distance per lineas diagonales.	Zenit distance per strias cochleæ.	Zenit distance corrected.	Name.	Striæ cochleæ in arc.	Lineæ diagonales—Striæ cochleæ.
h. m. s.		° ′ ″		° ′ ″		° ′ ″	″
8 23 7	bor.	4 47 50	108.65	4 53 50	24 Camelop.	4 47 52	— 2
———	bor.	3 5 20	69.93	3 11 20	25 Camelop.	3 5 32	— 12
26 41	bor.	4 21 35	98.83	4 27 35	26 Camelop.	4 21 55	— 20
———	bor.	4 48 10	108.90	4 54 10	(x)	4 48 32	— 22
30 29	bor.	5 11 5	117.58	5 17 5	29 Camelop.	5 11 30	— 25
33 40	bor.	8 10 55	185.46	8 16 55	31 Camelop.	8 11 5	— 10

Index error +6′ 0.

The field of Flamsteed's telescope being 80′, the star 24 *Camelopardalis* would have left the field $\frac{40'}{15}$ sec. $\delta = 4^{m}\ 49^{s}$ after transiting (supposing the transit wire to have been pretty near the center), that is at $8^{h}\ 27^{m}\ 56^{s}$ clock time. As the questioned star (x) was observed after $8^{h}\ 26^{m}\ 41^{s}$, when 26 *Camelopardalis* transited, Argelander's hypothesis seems not impossible. Still, of the $1^{m}\ 15^{s}$ that 24 *Camelopardalis* remained yet in the field, a part was certainly taken up by the readings for 26 *Camelopardalis*, so that the former must have been quite near the edge already when observed. That here is not the remark "*post transitum*," which Flamsteed probably never has omitted in such cases, seems to indicate rather a star *in transitu*. Of stars in right ascension between 26 and 29 *Camelopardalis*, and nearly upon the parallel of 24 *Camelopardalis*, the Durchmusterung has the following:

		m.	h. m. s.	° ′
Dm.	+ 56.1060	9.0	5 34 54	+56 27.0
Dm.	1063	9.5	37 9	31.5
Dm.	1064	9.5	37 50	31.0

altogether too faint for Flamsteed's telescope; but they, especially the two latter stars, should be watched as to variability. We must confess, however, that shifting the hypothesis from an "extinct" to a variable star, is but a very unsatisfactory expedient. I am rather inclined to suppose that in some way, now unaccountable, the zenit distance was recorded erroneously. Only about 20′ or 22′ farther north is 28 *Camelopardalis* (Dm. +56°.1059. $6^{m}.6$), which in 1696 preceded 26 *Camelopardalis* in right ascension by 5^{s}, hence transited near the limit assigned for the time, which itself was not recorded.

4) B. Fl. 864 (5^{m}).

The observation of this star stands (Historia Cœlestis, II, p. 411) between η *Geminorum* and 5 *Monocerotis*, thus:

	h. m. s.		° ′ ″		° ′ ″
1701, Feb.2	8 12 56	Geminorum η	29 4 0	658.82	28 56 40
	....	Monocerotis post transitum	58 2 0	1307.42	57 52 40
	16 2	[5]................................	57 49 20	1310.70	57 40 0

Index error — 9′ 20″

Already Baily remarked that there is a difference of 21′ between the reading *per lineas diagonales* and that *per strias cochleæ*. Indeed 1307.42 converted into arc make 57° 11′ 2″. The corrections proposed by Argelander are very plausible, viz: 58° 7′ 0″ for the *lineæ diagonales* and 1317.42 = 58° 7′ 27″ for the *striæ cochleæ*, since from the other stars here observed follows the difference (*lineæ diagonales* – *striæ cochleæ*) = –29″. The declination in the *British Catalogue* then ought to be corrected by – 5′, and the star is easily recognized as LL. 11805, which was also twice observed by Bessel.

We have W. 6^h 112, for 1825: $6^h\ 3^m\ 20^s.73$ =	90° 50′ 11″;	– 6° 31′ 7″.3
Reduction to 1690	– 1 38 24	+ 0 .8
W. 112 for 1690	89° 11 47;	– 6° 31 6
Brit. Cat. 861 (corrected δ by – 5′)	89 — —	– 6 30 40
Difference		– 26″

5) B. Fl. 913($6\frac{1}{2}^m$).

There can be no doubt but that the star, which in the British Catalogue passes under the name of 21 *Geminorum*, is identical with the preceding star or 20 *Geminorum*, and originated from an error of 1^m in the time. Historia Cœlestis, II, p. 294, $7^h\ 26^m\ 50^s$ ought to be corrected into $7^h\ 25^m\ 50^s$.

20 *Geminorum* is a double star, now about 19″ asunder, and the sequens is called by Piazzi and others 21 *Geminorum*, which, therefore, is different from what in the British Catalogue was called so. The components are, according to Piazzi, respectively 8th and 7th magnitude, but together they made to Heis the impression of a 6.7 magnitude star. Flamsteed's telescope hardly could separate them, especially if they were at his time still nearer together, as the proper motions in Mädler's Bradley seem to indicate. Reducing Br. 955 and 956 from the last-mentioned catalogue to 1690, we find

Br. 955:	93° 33′ 15.02″;	+ 17° 56′ 59″.91
Br. 956:	33 16.34	57 19 .03
or the middle,	93° 33 15.7	+ 17° 57 9.5

The British Catalogue has.... No. 912:	93 32 30	+ 17 57 15
No. 913:	93 48 30	17 57 40

and it is clear that the former right ascension is the correct one, and the latter must be diminished by 15′.

6) B. Fl. 1007 (7^m).

In the note to this star, which was observed by Flamsteed 1695, March 8, at $7^h\ 1^m\ 51^s$, Baily says: "I cannot find any star that will correspond with the position here given. In my list of Flamsteed's inedited stars, I have suggested that it might be Piazzi VI, 316; but on reconsidering and re-examining the subject, I am not now of that opinion." The star, however, exists still in its place, where Flamsteed observed it, and is $W_2\ 7^h$ 66 = Dm. + 17° 1498 (7^m). We have

$W_2\ 7^h$ 66, for 1825: $7^h\ 1^m\ 40^s.01$ =	105° 25′ 0.2″;	+ 17° 15′ 31″.3
Reduction to 1690 ...–	1 57 13.6;	+ 11 15 .1
W. 66 for 1690.........	103 27 47;	+ 17 26 46
British Catalogue 1007 is	103 27 5;	+ 17 26 30
W. – Fl...........	+ 42″	+ 16″

Argelander makes (A. N. No. 226, p. 162) a curious mistake. He says (ad. No. 118): "Stimmt vollkommen mit Piazzi VI, 316, wenn man die Durchgangszeit liest $7^h\ 1^m\ 31^s$ statt $7^h\ 1^m\ 51^s$." The position of Piazzi 6^h 346 is

for 1800:	104° 57′ 47″.2;	+ 15° 38′ 57″.0
Reduction to 1690:	–1 34 25. 0;	+ 9 0.2
Piazzi 346 reduced to 1690:	103° 23 22;	+ 15° 47 57

The place of the British Catalogue 1007, when corrected by — 20ˢ or — 5′ in right ascension, as Argelander proposes, is

103° 22′ 5″; +17° 26′ 30″
Difference (Pi.—Fl.) +1′ 17″; — 1° 38′ 33″.

In the Historia Cœlestis, II, p. 250, the observed zenit distance for B. Fl. 1007 is given as 34° 6′ 45″; for the star immediately following, which is 51 *Geminorum*, 34° 56′ 20″ (this should be, rather, 34° 55′ 20″, if corrected according to the reading *per strias cochleæ*, which here is right). The star No. 1007 was therefore *north* of 51 *Geminorum* by 49′ 35″, which is so nearly half of the difference between Pi. 346 and Flamsteed's star that, through applying it with a wrong sign, as it seems, Argelander was misled to the identification with Pi. 346.

7 and 8) B. Fl. 1198 (6^m) and 1199 (6^m).

These stars are but very imperfectly observed (see Historia Cœlestis, II, p. 287, 1696, January 23); the times were not recorded and the zenit distances read off only approximately *per lineas diagonales*. They transited

between B. Fl. 1232=7 Ursæ maj.=Dm.+61° 1070 (7, 5^m): 8^h 31^m 5^s; +61° 25′,6
and B. Fl. 1235=6 Ursæ maj.=Dm.+65° 673 (6, 0^m): 8^h 44^m 8^s; +65° 9′,0.

There are in the Durchmusterung zone for +49° between these right ascensions no other stars of sufficient brightness for possibly being seen in Flamsteed's telescope than the following, viz:

Dm. 1758 (8^m3), 1759 (6^m8), 1766 (7^m5), 1768 (8^m5), 1771 (8^m4), 1772 (7^m9), 1776 (8^m2).

Only the two brightest ones of them agree with the condition imposed by the zenit distances as far as recorded. Reduced to 1696 they are:

Dm. +49°.1759 (=LL. 17058—9=Pi. 8^h.131) : 8^h 21^m 40^s; + 49° 54′ 8
Dm. +49 .1766 (=LL. 17138 =Pi. 8.141) : 8 24 3 ; + 49 56,3,

which give the apparent zenit distances, *south* 1° 40′, while Flamsteed's readings were *aust.* 1° 30′,
and *south* 1 38, and *aust.* 1 35.

These two stars, therefore, well satisfy the roughly taken observations. Argelander had come to the same conclusion, from which Baily, however, dissents, without giving anything better in its place.

9 and 10) B. Fl. 1205 and 1212.

The stars in and around *Præsepe*, contained in the British Catalogue, with the right ascensions converted from arc into time, are shown in the first five columns of the following table:

No.	Fl.	Mag.	Brit. Cat. 1690.		Dm. No. and mag.		1690 computed.		Comp.—Fl.	
			h. m. s.	° ′ ″	°	m.	h. m. s.	° ′ ″	s.	′ ″
1193	33η	6½	8 11 45	+21 27 25	+20.2109	5.6	8 14 42.3	+21 27 22	— 3	— 0 3
1195	35	7	17 26	20 37 15	.2118	7.6	17 25.8	20 37 12	0	— 0 3
1196	. .	7	17 55	20 0 0	.2123	8.2	17 51.8	20 48 14	— 3	—
1205	. .	—	20 47	20 18 50	19.2053	7.2	19 56.4	20 18 54	—51	+ 0 4
1211	38	8	21 51	20 49 45	20.2149	7.0	21 49.5	20 50 6	— 2	+ 0 21
1212	*f*	—	22 2	20 46 10	.2150	7.3	21 58.4	20 35 54	— 4	—10 16
1213	39	6	22 14	21 4 30	.2158	6.7	22 11.8	21 3 59	— 2	— 0 31
1214	40	6	22 19	21 1 50	.2159	6.8	22 17.2	21 1 48	— 2	— 0 2
1216	. .	—	22 31	21 0 0	.2166	7.1	22 28.7	20 43 48	— 2	—
1217	41ε	7	22 40	20 36 20	.2171	7.2	22 36.0	20 36 22	— 4	+ 0 2
1218	42	7½	22 51	20 46 45	.2172	7.2	22 50.8	20 46 53	0	+ 0 8
1219	. .	—	23 1	20 38 45	.2175	7.7	23 4.9	20 38 38	+ 4	— 0 7

The next columns show the places of the same stars from modern catalogues, reduced to 1690; the last two columns, the differences with the British Catalogue. The magnitudes are from the Durchmusterung and from Prof. A. Hall's monograph upon this cluster.

If we correct the right ascension of Fl. 1205 by -51^s, that is, if we assume with Argelander, that on 1698, March 10, the time of transit was $8^h\ 27^m\ 43^s$ instead of $8^h\ 28^m\ 34^s$, the agreement of the position of this star with the modern determinations is perfect. Argelander's $+19^\circ.2053$ is LL. 16939—41, Pi. $8^h.112$, etc.

As to Fl. 1212, the right ascension of which is quite correct, Baily affirms from the MSS. that owing to the quick succession of the star's transiting there was, on 1696, March 18, some confusion in the entry of the zenit distances, so that opposite to this star very probably belonged the reading 30° 58′ 45″ instead of 30° 48′ 5″. The star Dm. $+20^\circ.2150$ is No. 65 of Professor Hall's list of the stars of *Præsepe*. We ought to hesitate the less to accept the proposed corrections, as Flamsteed's list thus is complete of the brighter stars of *Præsepe*, but only by including these two. There is no reason at all, therefore, to suspect here the disappearance of a star.

11) B. Fl. 1220.

There were observed by Flamsteed on 1703, March 11, six stars north of the zenit, which, in the Historia Cœlestis, II, p. 457, stand thus: we denote them, for the sake of reference, besides by the numbers of the British Catalogue, also by letters:

	B. Fl.	Clock time transit.			Zenit distance per lineas diagonales.			Per strias cochleæ.	Zenit distance corrected.			Striæ cochleæ in arc.			Lineæ diagonales-striæ cochleæ.	
		h.	m.	s.	°	′	″		°	′	″	°	′	″	′	″
a	1190	8	19	38	9	22	20	210.36	9	32	30	9	17	13	+5	7
b	1210		26	55	16	5	50	364.86	16	16	0	16	5	45	+0	5
c	1220		28	34	1	22	45	99.34	4	32	55	4	23	16	—0	31
d	1284		52	52	11	12	10	253.72	11	22	20	11	12	25	—0	15
e	1287		54	30	10	57	40	248.23	11	7	50	10	57	52	—0	12
f	1325	9	12	19	19	26	20	440.77	19	36	30	19	26	35	—0	15

Index error + 10′ 10″.

The conversion of the reading *per strias cochleæ* into arc shows, by the last two additional columns, that the zenit distance *per lineas diagonales* of the first star was read off too great by 5′.

The stars are easily recognized to be identical with the following:

	Number and magnitude Durchm.		Reduced to 1703.						Authority.
	°	m.	h.	m.	s.	°	′	″	
a	+60.1148	6.8	8	14	37	+60	55	52	LL. 16805, Ö. Argelander 9124, and by Argelander's observations at Åbo meridian circle.
b	67. 560	6.0	8	21	20	67	44	46	Fed. LL. 1371—2, Piazzi $8^h.137$, Ö. Argelander 9262.
c	55.1297	7.5	8	30	36	56	1	30	LL. 17350, Ö. Argelander 9357.
d	62.1054 –5	7.6	8	48	42	62	51	7	LL. 17990—1, Ö. Argelander 9644 and 7.
e	62.1058	5.0	8	50	28	62	36	33	16 c Ursæ majoris (from Mädler's Bradley).
f	70. 565	5.2	9	7	25	71	5	50	24 d Ursæ majoris (from Mädler's Bradley).

In order, however, to insure a good agreement the observed clock times of the first three stars need some correction. For the star *c*, which is the star put into question by Baily, we adopt Argelander's very acute suggestion, that the time probably was $8^h\ 34^m\ 28^s$, instead of $8^h\ 28^m\ 34^s$. Stars *a* and *b* were written down 1^m too late, which, at least for *b* is readily conceded, the seconds being 55. For more complete evidence the times thus corrected and reduced for rate of the clock to some middle epoch are compared with the computed right ascensions for 1703 in the following

table. The same table contains also the computed zenit distances, obtained by subtracting from the declinations the latitude ($\varphi = 51° 28' 38''$), the index error ($i = 10' 10''$), and the refractions, so that they are comparable directly with the readings *per lineas diagonales*. The stars are arranged here in the order of their zenit distances or declinations, which makes apparent a regularly progressing effect of the Quadrant's deviation upon the times of transit:

Star.	Observed time.			Reduced for rate from $8^h 30^m$.	t, observed time reduced.			$a-t$		$\delta-\varphi-i$			Retr.	Computed north zenit distance.			Zenit dist. (comp—obs.)
	h.	m.	s.	s.	h.	m.	s.	m.	s.	°	′	″	″	°	″	′	″
c	8	31	28	0	8	31	28	—3	52	4	22	42	— 3	4	22	39	— 6
a	8	18	38	—2	8	18	36	3	59	9	17	4	7	9	16	57	—23
e	8	54	30	+4	8	54	34	4	6	10	57	45	11	10	57	34	— 6
d	8	52	52	+4	8	52	56	4	11	11	12	19	12	11	12	7	— 3
b	8	25	55	—1	8	25	54	4	34	16	5	58	17	16	5	41	— 9
f	9	12	19	+7	9	12	26	5	1	19	27	2	20	19	26	42	+22

From this it is clear that the star Fl. 1220 (letter *e*) fits so well to the other stars as to exclude also in this case the hypothesis of the disappearance of a star.

12) B. Fl. 1232 (6^m).

In the note to this star Baily explains, from the inspection of the MSS., how Flamsteed, having failed to record the time of transit at the Quadrant, tried to supply the right ascension for the catalogue by an observation with the Sextant, but in so doing he took in its stead 5 *Ursæ majoris*. The right ascension in Flamsteed's British Catalogue—127° 46′ 30″—is therefore quite wrong, and much too large. Baily says further: "I cannot find any star in any catalogue that will correspond with the zenith distance observed by Flamsteed, and I therefore consider that the star does not exist." If we take from Argelander's north zones the place of Dm. + 61° 1070 ($7^m.5$), which is also Fed. LL. 1361, and reduce it to 1690, we find

Ö. Argelander's 9191 : 124° 16′ 31″ ; +61° 59′ 1″
Flamsteed's British Catalogue has (127 46 30); +61 59 0

The star indeed transited, as from the record of the observation on 1696, January 23, is required, soon after 4 π *Ursæ majoris*, and before the two imperfectly observed stars, which have been considered above under the head of 7) and 8). We are now enabled to fill up the gap between 3 and 6 *Ursæ majoris* in the column of clock time, on page 287 of the Historia Cœlestis, II, as follows:

B. Fl.	Star.	Clock time.		
		h.	m.	s.
1185	3 Ursæ majoris	11	13	34
1186	4 π Ursæ majoris		[14	56]
1232	7 (= Ö. Arg. 9191)......		[19	32]
1198	Pi. 8^h 131...............		[23	28]
1199	Pi. 8^h.141		[25	51]
1235	6 Ursæ majoris		31	51

which confirms the justness of the identification of stars B. Fl. 1232, 1198, and 1199.

13) B. Fl. 1486 (5^m).

This star, which in the British Catalogue passes under the name of 28 *Sextantis*, was observed 1702, February 28, at $10^h 48^m 36^s$, and Baily found in the original manuscript entry that the 4 (in 48^m) had been originally a 5. There can be no doubt that the time should be corrected by

$+2^m$, or that instead of 48^m should be read 50^m, and that it is an observation of 29 *Sextantis*, identical with Pi. $10^h.86$ (in Piazzi's catalogue erroneously called 28 *Sextantis*).

Piazzi $10^h.86$ for 1800	151° 49′ 13″;	— 1° 43′ 10″
Reduction to 1690	—1 23 58	+33 6
Piazzi 86 for 1690	153° 25′ 15″;	—1° 10′ 4″
British Catalogue 1486	152 52 15;	—1 10 25
Difference	+33′ 30″;	+ 21″
29 Sextantis, or B. Fl. 1191, is	153 23 45;	—1° 10′ 30

which differs from B. Fl. 1486 with the correction of $+2^m$, only 1′ 30″ in right ascension and 5″ in declination.

14) B. Fl. 1647 (7^m).

Argelander (A. N. No. 227, page 171) says:—"ist eine bis jetzt noch nicht bekannte Beobachtung des Uranus. Aus einer genauen Reduction folgt die Position 1714, Dec. 14., 17^h 54^m 57^s M. Z. Gr. 11^h 29^m $1^s.94$; + 4° 11′ 6″.5, sehr schön mit den übrigen Flamsteed'schen Beobachtungen dieses Planeten übereinstimmend."

Baily, on the contrary, in the note to this star, remarks: "I cannot find any star that will correspond with this observation. Mr. Argelander thinks that it may be Uranus, whose position on that day, at 17^h 54^m 57^s mean time at Greenwich, was 11^h 29^m $1^s.94$ (=172° 15′ 29″) and D= +4° 11′ 6″.5; but the great difference in the declination is against this supposition."

For the time of observation concluded by Argelander I have computed from the solar tables of Hansen and Olufsen:

The sun's apparent trop. longitude $\odot = 262° 59′ 23″.9$
The sun's apparent trop. latitude $\sigma = +0^s.27$
The sun's radius vector log. R = 9.9928995
Also the obliquity of the ecliptic $\varepsilon = 23° 28′ 29″.2$
And the nutation in longitude = + 14″.56.

For the same instant, but diminished by 2^h 11^m 34^s, the time required by the light to travel from Uranus (for which the distance was taken from a preliminary computation), I derived from Newcomb's Tables:

The longitude of Uranus in reference to the mean equinox 168° 9′ 22″.67
to which added the nutation in longitude found from the solar tables + 14.56
gives the true or apparent longitude of Uranus $\lambda = 168$ 9 37.23
Further the latitude of Uranus $\beta = +0$ 46′ 6″.44
and the radius vector of Uranus log. r = 1.2620674.

With these data we obtain:

The apparent right ascension of Uranus $\alpha = 172° 15′ 47″.5$
The apparent declination of Uranus $\delta = +1$ 11 5 .9
Distance from the earth log. $\Delta = 1.2607247$,

and the small correction for parallax, to be added to computed declination, will be = − 0″.36.

Hence the final comparison stands thus:

Tabular computed place	11^h 29^m $3^s.17$;	+ 4° 11′ 5″.5
Observed (Argelander's reduction)	11 29 1 .94;	+ 4 11 6 .5
Difference (C—O)	+1 .23	—1 .0

Argelander's assertion of this being an observation of URANUS ("very finely agreeing with the other observations of this planet by Flamsteed") is therefore completely vindicated. Perhaps misled by Baily, no notice has been taken of this observation in the recent tables of Uranus.

15) B. Fl. 1686.

Flamsteed compared, on three days in April, 1708, the planet Jupiter with 3 stars of *Virgo*, as seen in the Historia Cœlestis, II, p. 518, thus:

	April 2.		April 5.		April 6.	
	Time.	Zenit dist.	Time.	Zenit dist.	Time.	Zenit dist.
	h. m. s.	° ′ ″	h. m. s.	° ′ ″	h. m. s.	° ′ ″
Jupiter	10 8 48	47 16 50	9 56 23	47 10 20	9 50 16	47 8 20
b Virginis	16 52	46 11 10	10 5 27	46 10 50	10 1 38	46 10 50
♄ (*i. e.*, 10) Virginis	26 33	47 56 50	15 9	47 55 40	11 23	47 55 50
....	30 51	47 34 40	19 27	47 34 40	14 17	47 34 40

It is quite manifest that the last star observed on April 6 was the same as that observed on the other days, viz: Pi. $12^h.16$, No. 1688 of the British Catalogue. Already Miss Herschel noted this, but that the clock time was put down $1^m\ 24^s$ too early, and should read $10^h\ 15^m\ 41^s$ instead of $10^h\ 14^m\ 17^s$. Baily's inspection of the MS. showed the print in conformity with the original. How the mistake may have originated it is quite useless now to speculate about; but it seems not less unreasonable to suspect here the observation of a star now lost.

16) B. Fl. 1910 (6^m).

There is no star of the sixth magnitude in the place entered in the British Catalogue under the name of 91 *Virginis*, and which comes from an observation made on May 13, 1703.

We compute from Mädler's Bradley for 1703 the right ascensions and apparent zenit distances of:

	α	δ	$\varphi-\delta$	Refr.	App. Z.
	° ′ ″ h. m. s.	° ′ ″	° ′ ″	′ ″	° ′ ″
84 *o* Virg.	202 2 24=13 28 10	+5 3 22	46 25 16	−1 1	46 24 15
92 Virg.	205 20 29=13 41 22	+2 31 16	48 57 22	−1 6	48 56 16
93 *τ* Virg.	206 38 22=13 46 33	+3 0 5	48 28 33	−1 5	48 27 28

On page 461 of the Historia Cœlestis, II, are reported the following observations, in agreement, as Baily assures us, with the MS. transcript, the original entry being lost:

	Clock time.	Zenit distance corrected.	Reduction for rate.	T	$a-T$
	h. m. s.	° ′ ″	s.	h. m. s.	h. m. s.
o Virg. .	9 29 52	46 24 10	0	9 29 52	+3 58 18
[Star *x*] .	41 10	48 56 30	+2	41 12	60 10
τ Virg. .	48 12	48 27 45	+3	48 15	58 18

Here the zenit distance of star *x* agrees with that of 92 *Virginis*.

Reducing the clock time for sidereal rate, as in the 4th column, and subtracting the resulting T from the respective right ascensions of the preceding table, the column $a-T$ is formed. The variance shown here would disappear by assuming a correction in the recorded time of $+1^m\ 52^s$ (or of $1^m\ 50^s$, or, perhaps, roundly 2^m), and the star therefore very likely was 92 *Virginis*. No further conjecture regarding the origin of the error can be made, since, as said, the book with the original entry for this time is lost.

17) B. Fl. 1922 (6^m).

This star, observed on May 13, 1704, can hardly be any other than 10 *Bootis*, which is near the place. Flamsteed observed on two consecutive days a sequence of stars in Bootis, beginning on May 13 with the questioned star [x], and on May 14 with 10 *Bootis*. The observations of these are, in the Historia Cœlestis II, pages 477 and 478, reported thus:

Date.	Name.	Clock time.	Zenit dist. per lineas diagonales.	Zenit dist. per strias cochleæ.
		h. m. s.	° ′ ′	° ′ ″
1704, May 13..	[x]	9 43 28	27 53 0	631.92 [=27 53 22 in arc]
May 14..	10 Bootis..	9 38 12	28 28 50	645.57 [=28 29 27 in arc]

From 11 stars, the transits of which were taken on both days, the rate of the clock in a sidereal day = -4^m 48^s, so that, according to the observation of May 14, 10 *Bootis* would have transited on May 13 at 9^h 42^m 0^s clock time. The book with the original entries for these years unfortunately does not exist; Baily found only the copies. To reconcile both days' observations, I imagine the entry of May 13 originally may have stood thus:

9 43 28 27 35 ..., 92

which was copied in the way as written above, the whole number of the *striæ cochleæ* being filled in to correspond with the *lineæ diagonales*. But the figures should have been distributed, and the *striæ cochleæ* supplemented rather, as follows:

9^h 43^m 0^s 28° 27′ 35″ 644.92 [= 28° 27′ 44″ in arc].

The substitution here of 35 for 53 is necessary, since the division was read off only to 5″. Of such interchange of two figures Flamsteed was not quite free, as we see from other examples. The error in the clock time of 1^m likewise is nothing extraordinary, and the identity of the star [x] with 10 *Bootis* thus becomes complete.

Argelander's hypothesis that Flamsteed observed a star *north* of the zenith, which would lead to the star Fed. LL. 2349 ($5\frac{1}{2}^m$), is contradicted by the fact that the declination +79° 12′ would bring it far beyond the limit of the constellation *Bootes*, where it is distinctly stated that it was. The parallelism of the two series of May 13 and 14 also speaks against the surmise that the word *bor.*, which is neither in the print nor in the manuscript, as Baily assures us, had been omitted only by forgetfulness.

18) B. Fl. 2120 (—).

In the Historia Cœlestis, II, p. 116, are the observations:

Date.	Name.	Clock time.	Zenit dist. per lineas diagonales.	Zenit dist. per strias cochleæ.	Striæ cochleæ converted into arc.
		h. m. s.	° ′ ″		° ′ ″
1691, June 4..	[x]	5 52 45	23 29 20	532.06	23 29 33
	α Coronæ..	53 56	23 43 20	537.42	23 43 44

where the adjoined column shows the agreement between the two readings of zenit distance.

There exists no large star so near to *α Coronæ*, as already Burckhardt pointed out (Zach's Mon. Corr. 26, p. 579). The difference in time from *α Coronæ*, as Argelander remarks, equals exactly the difference in right ascension of *θ Coronæ* from the same star; and also the zenit distance brings us quite upon this star, if we diminish by 100 the *striæ cochleæ* reading, or for 532.06 read 432.06, which converted into arc is =19° 4′ 52″. We cannot hesitate in taking this

for the solution. For the difference *θ Coronæ* — *α Coronæ* for the epoch of 1690 is indeed $\Delta\alpha = -1^{m}\ 10^{s}$ and $\Delta\delta = +4^{\circ}\ 39'$.

Baily found in the original MS. entry the statement that "Mr. Clowes *alone* made the observation." In the short interval of $1^{m}\ 11^{s}$ between the transits of the two stars, Mr. Clowes had scarcely the time, with writing and noting the clock, to make *both* the readings. Probably he contented himself with the *striæ cochleæ* reading, but in the haste made a mistake of 100 units. Modeled from the wrong figure, 532.06, arose afterwards, it seems, the strange zenit distance in the column *per lineas diagonales*.

19) B. Fl. 2335 (5^{m}).

In the Catalogus Britannicus, in the third volume of the Historia Cœlestis, on page 53, the 55th star of Hercules is united with the 54th by a circumflex, with a figure for the magnitude common to both. This, together with what Baily (in the note to No. 2335) says of the MSS., shows that Flamsteed considered them as one and the same star. The single observation, of the zenit distance only, on April 8, 1703, is probably nothing but a repeated measure of the preceding zenit distance, which is of 54 *Herculis*. The number 55 *Herculis*, therefore, must be stricken out in the catalogue; it cannot be counted in the class of "observed and disappeared" stars.

20) B. Fl. 2441 (6^{m}).

The star, that in the British Catalogue passes under the name of 65 *Ophiuchi*, could not be found by Piazzi; and Airy, who, at Baily's request, looked out for it, had no better success. In the observation of 1691, May 6, probably a mistake was made in *both* the co-ordinates. If we correct the clock time by $+1^{m}$ (perhaps better still by $+1^{m}\ 10^{s}$) and the zenit distance by $-50'$, *i. e.*, if Historia Cœlestis, II, page 112, for $14^{h}\ 10^{m}\ 58^{s}$, we read $14^{h}\ 11^{m}\ 58^{s}$ (or perhaps $14^{h}\ 12^{m}\ 8^{s}$) and for $69^{\circ}\ 24'\ 30''$ . . . . $68^{\circ}\ 34'\ 30''$, the place is in perfect harmony with 6 *Sagittarii*. Indeed, by determining the constants from 15 other stars observed on that day, I find when applying the proposed corrections,

the observed place, reduced to 1690.0: $17^{h}\ 43^{m}\ 14^{s}$ (or 24^{s}); $-17^{\circ}\ 5'\ 52''$, while 6 *Sagittarii*, as derived from Mädler's Bradley, is for the same epoch: $17^{h}\ 43^{m}\ 24^{s}$; $-17^{\circ}\ 5'\ 58''$.

The only difficulty remaining is to find an explanation for the figures of the column *per strias cochleæ*, which would have to be altered into 1554.62 about. But the agreement resulting from the very simple and unstrained changes proposed is so close, and on the whole the spurious place so near to the corrected one, that about the identity with 6 *Sagittarii* there can scarcely be a doubt. The number 65 *Ophiuchi*, therefore, must be erased from the Catalogue.

21) B. Fl. 3150 (7^{m}).

Flamsteed's star 80 *Aquarii* has often been observed in more recent times. In the catalogues it is: LL. 45022, Pi. $22^{h}.279$, W. $22^{h}.1133$, R. 10795, Lam. 4695, Glasgow 6039. All these indicate no pronounced proper motion, and give

the position for 1690.0: $22^{h}\ 45^{m}\ 27^{s}.8$; $-6^{\circ}\ 21'\ 58''$
No. 3150 of the Brit. Cat. is: $22^{h}\ 44^{m}\ 30^{s}$; $-6^{\circ}\ 22'\ 35''$

so that it is clear the star exists, but Flamsteed's time of transit was 1^{m} in error, and the Catalogue right ascension must be increased by $+15'$.

The name 80 *Aquarii* should be reinstated in modern catalogues, for ex. to Heis. No. 99. I do not find that the star has been reobserved at Greenwich since Flamsteed's time.

22) B. Fl. 3213 (6^{m}).

There is no trace of actual observations for the position of 3 *Cassiopeæ*, which seems to have originated, as we understand from Baily's examination of the MSS., in Flamsteed's computations of No. 3224. Hence we have no reason to suspect here a "disappeared" star.

The conclusions arrived at in the foregoing discussions are recapitulated in the following tabular form:

	No. B. Fl.	Name in British Catalogue.	Result of investigation.
1	314	28 Arietis	26 Arietis.
2	639	100 Tauri	Bradley 686.
3	756	27 Camelopardi.....	28 Camelopardalis (or a variable?).
4	864	Monocerotis	LL. 11805.
5	913	21 Geminorum......	20 Geminorum.
6	1007	Geminorum	W_2 7^h. 66 (Dm. + 17°.1498).
7	1198	Ursæ majoris....	LL. 17058–9 = Pi. 8^h.131.
8	1199	Ursæ majoris....	LL. 17138 = Pi. 8^h.141.
9	1205	Cancri	LL. 16739–41, etc. (Dm. + 19°.2053).
10	1212	Cancri	*Præsepe*, Hall No. 65 (Dm. + 20°.2150).
11	1220	Ursæ majoris....	LL. 17350 = O.Arg. 9357.
12	1232	7 Ursæ majoris....	O. Arg. 9191 (Dm. + 61°.1070).
13	1486	28 Sextantis........	29 Sextantis.
14	1647	Leonis	*Uranus!*
15	1686	Virginis	B. Fl. 1688 = Pi. 12^h.16.
16	1910	91 Virginis	92 Virginis.
17	1922	Bootis	10 Bootis.
18	2120	Coronæ bor......	θ Coronæ borealis.
19	2335	55 Herculis	54 Herculis.
20	2441	65 Ophiuchi	6 Sagittarii.
21	3150	80 Aquarii	80 Aquarii.
22	3213	3 Cassiopeæ	Obs.? substituted in computation of No. 3224.

Although the evidence of identity could not be made equally strong for all of the twenty-two cases left unsolved by Baily, nevertheless it is manifest that no reason exists to suppose any of the stars seen by Flamsteed to have been lost, or become extinct in the nearly two hundred years since elapsed. All the stars in the British Catalogue have now been accounted for, as well as the positions permit.

In concluding I may be allowed a remark, suggested while occupied with examining Flamsteed's observations. The astronomical world has not yet done justice to the sacrificing zeal, the industry, the honest work of the first Astronomer Royal. The star catalogue is still in the same crude state as it came from the hands of the author. Baily has done much in rectifying errors of computation, discarding wrong positions, rectifying others; but still, the *British Catalogue* of Baily is yet the old *Catalogus Britannicus*, the product of an age when the methods of reduction were in their infancy, the elements for the same imperfect, aberration and nutation even not yet discovered. The catalogue does not represent the observations with equivalent accuracy. Already Baily urged strongly a new reduction. "I do not despair," he says, "of its being accomplished at some future time, since those observations have much intrinsic value." But, since these words were uttered, over half a century has elapsed again, and however carefully the 70 volumes of Flamsteed's MSS. may be preserved at Greenwich, with time the paper must molder and the writing become more and more illegible. Baily, Argelander, and Krueger have done valuable preparatory work for a reduction. Has England no young astronomer ambitious to undertake the task?

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

ELEVENTH MEMOIR.

CORRIGENDA IN VARIOUS STAR CATALOGUES.

CORRIGENDA IN VARIOUS STAR CATALOGUES.

BY C. H. F. PETERS.

READ NOVEMBER 12, 1885.

The frequent use of star catalogues for various purposes (planetary and cometary comparisons, zone observations, charts, &c.) has led to the occasional detection of errors in them, which are here collected. Their publication may be useful and save much trouble sometimes to other observers, especially to those who, like the writer, have not at their disposition a meridian circle for verifying a position needed, but must rely upon the correctness of the positions published.

I have excluded here the lists for the Harvard zones, which have been published elsewhere (Annals of the Astronomical Observatory of Harvard College, Vol. XIII, pages 188–208), those for the Washington zones, which were communicated in MS. to Professor Holden, now engaged upon cataloguing the zones, and those for Lamont's publications, which at the present are undergoing a revision and reordination by the astronomers of the Munich observatory.

To discard the errors from catalogue positions has not only a practical utility, but is a step towards a more and more truthful representation of the skies. Therefore we see an Argelander, perhaps the best informed in our age of the starry heavens, revising step for step the "Histoire céleste," correcting Baily's catalogue of the same, then examining with equal endurance Bessel's zones, &c. Following the example of this master critic, I have, for the lists here presented, not been content with the simple fact of an error, but mostly turned to the original observations, when accessible, in order to discover the source of error.

These contributions, from the way in which they originated, as stated, make of course no claim of completeness. Most of the errors indicated, however, will be found hitherto unknown. The corrections proposed are, I believe, wholly reliable; where not quite certain, a *prob.*, or the sign of a query, has been adjoined.

I.—*Corrigenda in Oeltzen's Catalogue of Argelander's Southern Zones.*

No.	Col.	
70	α	For 38s.99 read 48s.99 (or perhaps 49s.99)
135	α	10s.20 read 0s.20.
774	δ	25′ read 23′ (error of Cat., Z. 336.13 right).
790	δ	18′ read 19′.
869	α	36s.74 read 58s.48 (—error in Z. 266.16 in the reduction to middle wire).
972	δ	19′ read 18′ (error in Cat., Z. 331.64 right).
1589	α	23s.38 read 13s.38 (Z. 313.30 wire 3 right, while wire 2 too large by 10s, which error was retained in reducing to the middle wire.)
1773	α	36m read 37m (misprint in Z. 322.10).
1872	δ	23′54″ read 24′4″.
2007	δ	33°33′50″.5 read 23°43′54″.1 (—error of print of 10° in Cat., and besides error of 10′ in circle reading in Z. 313.67).
2571	δ	36′ read 34′.
2761	δ	55′ read 54′.
2769	δ	43′38″.1 read 53′40″.4 (error of 10′ in Micr. Z. 325.95).
2858	α	12s.28 read 15s.86 (—wire 4 in Z. 350.7 was right).
3084	δ	For 36′ read 37′.
3120	δ	3′48″.2 read 4′8″.2.
3123	δ	80′ read 20′ (misprint).
3492	δ	54′ read 53′.
3819	α	20s.31 read 30s.31.
4091	δ	23′13″.0 read 33′15″.3 (—error in Z. 274.45).
4202	δ	59′ read 50′ (prob. misprint; Z. correct).
4402	δ	19′ read 16′ (—Z. 323.105 is correct).
4435	α	28s.50 read 39s.03 (one wire interval, as suspected already by Argelander himself).
4885	α	Perhaps too great by 19s.7 (or the interval between wires 3 and 4). The correction would bring agreement with Lamont 65.
5022	δ	For 11′ read 12′ (prob.).
5532	δ	37′ read 38′.
6020	α	45s.86 read 55s.86, by correcting an error committed in reducing Z. 282.26 to middle wire. Wash. Mur. Z. 216 gives still 10s more.
6260	δ	27′ read 26′.
6357	α	4m read 5m (misprint).

I.—*Corrigenda in Oeltzen's Catalogue of Argelander's Southern Zones*—Continued.

No.	Col.	
6377	α	For 33^s.41 read 32^s.41.
6580	α	Wash. Z. Mur. 101.2 and Mer. C. 145 69 agree to make the AR. 2^s greater (—rightly reduced from Z. 282.49).
6964	α	For 15^s.44 read 27^s.04 (—in Z. 282.66 for wire 7 read wire 6).
7011	α	39^s.32 read 29^s.32.
7284	δ	17°53′39″.6 read 18°3′40″.8 (—error of 10′ suspected already by Argelander in note to Z. 278.118).
7301	δ	23° read 22° (misprint; in Z. right).
7496	δ	22° read 23° (misprint; in Z. right).
7535	α	44^m read 45^m
8349	δ	19° read 29° (misprint).
8697	α	27^m 1^s.62 read 26^m 51^s.27 (error of one wire interval in Z. 368.48).
8754	δ	40′ read 42′.
9303	α	57^m read 58^m (error in Zone, alike the foll.).
9306	α	57^m read 58^m.
9307	α	57^m read 58^m.
9823	δ	9′ read 8′ (error in Z. 363.37).
9827	α	36^s.21 read 26^s.21.
10112	δ	15′ read 16′.
10220	α	48^m read 47^m (error in Z. 361.27).
10280	δ	2′ read 3′ (?).
10298	δ	15′14″.1 read 5′11″.9.
10583	δ	25′ read 24′.
10594	δ	30′ read 31′.
10805	α	29^s.93 read 28^s.93 (?—comp. Bonn B. VI).
11026	δ	Is 20′ farther north than LL. and Lamont.
11193	α	For 34^s.45 :: read 44^s.45 (Wash. Z.).
11259	α	28^s.78 read 58^s.78 (error of Cat.; Z. right).
11319	α	13^m read 14^m (also Z. 401.39 to be corr. by + 1^m).
11433	δ	24°43′36″.2 read 25°43′49″.8 (error of 1° in Zone).
11491	δ	10′ read 12′.
11621	α	42^s.40 read 52^s.34 (in Z. 376.15 wire 6, not 7).
11674	δ	5′2″.3 read 15′4″.2.
11919	δ	14′ read 13′.
12025	δ	16°47′18″.1 read 17°47′25″.6 (error of 1° in Z. 367.127).
12029	δ	16°42′6″.8 read 17°42′14″.2 (error of 1° in Z. 367.126).
12066	δ	59′ read 58′.
12429	δ	25° read 24° (error typ.).
12594	δ	28′ read 27′.
12646	δ	28′29″.6 read 38′33″.6 (error in Z. 383.4).
12752	δ	51′ read 52′ (obs. in Bonn B., VI).
12758	δ	57′ read 58′.
12909	α	22^m read 23^m (error of Cat.; Z. right).
12998	α	Is about 30^s too small (W. 13^h.509, and Ham. Coll. Z.).
13269	α	For 27^s.19 read 17^s 19 (the star occurs 3 times in Wash. Z.).
13402	δ	50′ read 51′ (error in last column of Z. 206 30).
13652	δ	37′52″.0 read 27′53″.0.
13747	δ	34′ read 35′.
13779	δ	49′37″.1 read 39′35″.6 (the star occurs also in Bonn B., VI).
14295	δ	29′ read 39′.
14296	δ	28′ read 38′.
14362	α	+20^s? Y. 6257 and Wash. Z. Mur. 167 10 agree.
14480	δ	For 6′ read 5′ (error already in Z.).
14616	δ	39′ read 38′ (star is duplex).
14622	δ	18°0′ read 17°58′.
14703	δ	57° read 17° (misprint).
14707	δ	39′ read 38′ (— also obs. in Bonn B., VI).
14720	δ	For 8′40″.6 read 9′10″.6, about.
14803	α	Needs a correction of about —10^s. Perhaps Z. 212.2 was observed wire 7, not 6, which would make the AR. 15^s.59 instead of 26^s.05.
14807	α	For 35^m read 36^m.
14846	α	37^m read 38^m.
14862	δ	29′ read 28′.
14863	α	38^m read 39^m.
14907	α	16^s.34 read 26^s.34 (error in Z.; the star = No. 14911).
14921	α	26^s.01 read 36^s.01 (the star = No. 14927).
15003	δ	38° read 28° (misprint).
15043	α	52^s.93 read 53^s.93 (prob.).
15082	δ	24′.51″.7 read 34′53″.2 (error of col. 7 in Z. 208.104).
15149	δ	45′ read 44′ (— also obs. in Bonn B., VI).
15150	α	54^m read 55^m (—error of 1^m in Z. 297.65. The star is identical with No. 15166).
15259	δ	No star exists in that position. By correcting in Z. 205.69 the microsc. reading by +50′, the declination of the Cat. will result 15°42′10″.4 instead of 16°32′15″.7. The star therefore identical with No. 15260.
15266	δ	In Z. 207.132 is an error of 1°. The Cat. declination for 18°50′0″.9 read 17°49′53″.1.
15292	δ	For 3′ read 2′.
15489	δ	13′13″.5 read 3′8″.0.
15768	α	2^{s}31 read 43^s.38 (—wire was 3, as Arg. suspected in the note to the star Z. 213.40).
15865	α	34^m read 35^m.
15944	δ	3″.3 read 13″.3 (prob., though rightly reduced from Z.).
15989	δ	33′23″.9 read 23′21″.6 (Y. 6937, and Tacchini 638).
16218	δ	32′46″.8 read 42′49″.1 (obs. also by Arg. in Bonn B., VI).
16242	α	Arg., in note to Z. 389.48, says: "10^s?" But according to Was. Z. Mur. 173.33, and Mer. Cir. 97.114, the AR. needs rather a correction of about —40^s.
16287	α	For 33^s 93 read 23^s.56 (in Z. 392.23 probably wire 3, and not wire 2).
16332	δ	30′ read 29′ (confirmed by Bonn B., VI).
16431	δ	19°13′44″.1 read 20°13′52″.5 (Arg., Bonn B., VI).
16585	—	Found no star in this place. An 8.9^m star is about 4′50″ farther north and 7^s.5 less in AR.
16668	δ	For 48′ read 38′ prob. (Bonn B., VI).
16916	δ	37′ read 38′.
16934	δ	14′ read 4′ (—as Argelander already suspected in note to Z. 213.102).
16943	δ	5′ read 6′ (Ham. Coll. Z.).
16955	α	25^m read 24^m (mistake in red. to middle wire).
16982	—	Found no star in this place, though often looked for. The stars preceding and following it in Z 393 are correct.
17061	α	For 32^{m}1^s.84 read 31^{m}51^s.91 (—the difference of 1 wire in Z. 300.126, thus agreeing with observation in Bonn B., VI).
17075	α	39^s.40 read 29^s.09 (prob. error of 1 wire dist.).
17087	α	To be corr. by +5^s.56, which makes the star identical with No. 17093. Argel. suspects the error in his note to Z. 393.81.
17090	δ	29″.5 read 9″.5 (error of reduction).
17231	α	39^m 44^s.08 read 40^m 4^s.95 (—in Z 222.34. the wire was 3, not 4).

I.—*Corrigenda in Oeltzen's Catalogue of Argelander's Southern Zones*—Continued.

No.	Col.	
17236	α	Is about 8^s too great.
17309	δ	For 26′ read 27′ (misprint; Z. right).
17404	δ	55′ read 45′ (Bonn B. VI correct).
17523	δ	41′ read 42′ (obs. in Bonn B. VI).
17607	δ	42′21″.9 read 32′16″.7 (error of reduction in Z. 223.41).
17746	δ	24°6′25″.8 read 23°56′23″.5.
17790	α	45^s.86 read 43^s.86 (— error of reduction to middle wire in Z. 217.80).
18155	δ	37′ read 38′.
18279	δ	53′38″.9 read 43′37″.0.
18288	δ	17°44′1″.4 read 16°43′57″.0 (= LL. 34124. In the position of the Cat. no star is to be seen. There is an error of 1° in Z. 391.166).
18429	δ	49′47″.5 read 39′46″.3 (also obs. in Bonn B. VI).
18774	δ	54′ read 55′.
19058	δ	38′37″.3 read 28′35″.1 (— error of reduction Z. 220.179).
19109	δ	57′ read 27′ (error of Cat.; Z. correct)
19370	α	10^{m}6^s.39 read 9^{m}45^s.30 (in Z. 308.133 the wire should be 7, not 5).
19371	α	6^s.94 read 12^s.91 (error perhaps from writing the time 10^s.0 for 16^s.0 in Z. 240.13).
19416	δ	51′ read 52′ (Lamont, and Bonn B. VI).
19652	δ	34′20″.0 read 44′21″.6 (Wash. Z., and Bonn B. VI).
19910	δ	37′ read 36′.
20007	δ	42′ read 44′.
20009	δ	41′23″.8 read 31′22″.0.
20201	α	38^s.63 read 58^s.63 (in Z. 244.42 the wire should be 4, not 5).
20260	δ	To be corrected by —10′: the preceding number is right, as shown by LL. and Lam. observations.
20368	α	An observation made at Berlin, A. N. No. 1637, has the AR. smaller by 10^s.
20542	α	For 41^s.42 read 31^s.42 (— error of Cat.; Z. right).
20555	δ	20′ read 29′ (misprint).
20617	α	26^m read 27^m (— error of Cat.; in Z. right. The star thus appears to be identical with 20632).
20688	δ	34′ read 35′ (— error of col. 7 in Z. 311.82).
20727	—	This star was not found in the sky.
20810	α	Needs a correction of +10^s, or perhaps of +9^s.82, the time interval between wires 2 and 3 in Z. 249.82.
20978	α	For 24^s.44 read 22^s.44 (— error of Cat.; Z. right).
20990	α	Is about 4^s too small; also in Zone.
21025	α	For 51^s read 52^s (error in Z. 247.95).
21036	α	52^s read 53^s (error in Z. 247.97).
21041	α	52^{m}43^s read 53^{m}33^s (error in Z. 247.96).
21075	α	24^s.83 read 34^s.83 (error of Cat.; Z. right).
21214	δ	5′ read 53′ (misprint).
21252	α	26^s.86 read 27^s.68.
21371	δ	20° read 30° (misprint).
21403	δ	20°49′51″.7 read 19°49′42″.6. There is no star in the uncorrected position, and the Z. 255.23 is 1° in error.
21434	α	22^m read 21^m.
21436	α	22^m read 21^m.
21436	δ	56′ read 51′.
21439	δ	4′ read 5′ (— error of Cat.; Z. right).
21660	α	40^m read 41^m (error in Z. 245.63).
21727	δ	21′ read 20′ (error of Cat.; Z. right).
21808	α	59^s.43 read 17^s.63 (in Z. 234.96 the wire should be 5, not 3, which makes 41^s.80 difference).
21909	α	48^s.36 read 38^s.07 (corrected by interval of wires 5 and 6 in Z. 254.95, to agree with Y. and Lam.).
22158	α	12^s.23 read 33^s.09 (error in reduction to middle wire in Z. 257.32).
22162	δ	44′46″.8 read 54′47″.9 (Ham. Coll. Z.).
22172	α	29^s.27 read 39^s.27 (error of Cat.; Z. right).
22212	α	24^m read 25^m (error in Z. 265.29).
22217	α	24^m read 25^m (error in Z. 265.30).
22220	α	25^m read 26^m (error in Z. 265.31).
22224	α	25^m read 26^m (error in Z. 265.32).
22291	δ	48′54″.7 read 38′50″.6.
22420	α	40^m read 41^m (error in Z. 250.18).
22430	α	41^m read 42^m (error in Z. 250.19).
22502	δ	45′ read 47′ (error in Cat.; Z. right).
22577	δ	51° read 15° (misprint).
22716	α	30^s.34 read 20^s.34.
22794	δ	19′50″.5 read 59′55″.2 (— error arising from a misprint in col. 7 of Z. 253.63).
22889	δ	14°37′54″.3 read 15°38′0″.0 (in col. 7 of Z. 250.68 there is a misprint of 1°).
23063	δ	23° read 24° (misprint in Cat.).
23141	α	49^m read 50^m.
23165	δ	43′2″.6 read 53′5″.5.
23193	δ	27′6″.5 read 37′9″.4.
23230	α	29^s.51 read 39^s.72 (error in reduction to middle wire Z. 269.34).
23247	α	59^m read 60^m (error of Z. 250.106; also the AR. of 107 and 108 of the same zone, and hence No. 1 and No. 16 of the Cat. ought to be increased by 1^m).

Among errors of smaller amount, not rare is that of 1^s in AR., which is more difficult to be found out. It would be desirable that in future no zone work be undertaken without the use of a chronograph, as the principal source of this error lies in miscounting the clock-beat. Many of the doubtful cases have been cleared up by Argelander himself, by a reobservation, in the 'Bonn B.,' Vol. VI. A few others are here appended.

Errors of about 1^s in right ascension.

No.	
14087	For 38^s.33 read 39^s.33 prob. (LL. and Lam.).
14858	54.25 read 55.25 (3 obs. Ham. Coll. Z.).
16116	Between 1^s and 2^s too small; the star was observed at Berlin, A. N. No. 1637.
16899	For 8.54 read 9.54 (obs. Ham. Coll.).
16971	59.28 read 60.28 (Wash. Z. and Ham. Coll. Z.).
17325	35.59 read 34.59 (Wash. Z. and Ham. Coll. Z.).
17822	For 49.35 read 48.35 (error of Cat.; Z. correct).
19055	18.19 read 19.19 (Lamont and Wash. Z.).
19413	59.29 read 60.29.
20606	13.73 read 12.73 (error of Cat.; Z. right).
21012	3.18 read 4.18 (the star is 20 Capricorni).
21079	50.88 read 51.88 (the star is η Capricorn).
21939	53.65 read 52.65 (R. 9859 and Ham. Coll. Z.).

II.—*Corrigenda in the catalogues of volume VI of the Bonn observations.*

Page.	Star.	Col.	
1	— 1°.2534	α	For $19^s.91$ read $20^s.91$.
10	— 0°.2611	α	For $36^s.51$ read $37^s.51$ (the number is not 2610.)
13	+ 0°.448	δ	For 38′ read 40′ (prob.).
16	0°.2749	α	Perhaps 10^s too small (comp. Harvard Z 4 and 5, No. 71.)
20	1°.282	δ	For 25′ read 24′ (Ham. Coll. Z.)
32	3°.43	α	For $31^s.82$ read $32^s.82$ (prob.).
32	3°.215	α	For 25^m read 24^m.
50	6°.2359	α	For $9^s.22$ read $10^s.22$ (prob.).
50	6°.2372	α	For $48^s.74$ read $47^s.74$ (prob.).
50	6°.2408	δ	For 57′ read 59′.
55	7°.2455	α	Seems from 1^s to 2^s too small.
55	7°.2492	δ	Is about 20″ too far north.
61	8°.2556	α	For $28^s.50$ read $29^s.50$ (prob.).
65	+ 9°.120	α	For $51^s.82$ read $21^s.82$.
67	9°.2244	α	For $13^s.57$ read $3^s.57$ (in Dm. is the same mistake.)
70	10°.2228	—	Argelander says in the note that this star is double. I have never seen it so, though often looked for it.
83	13°.2272	α	For 26^m read 24^m. The star is Schj. 3860. The number is not 2274.
83	13°.2285	α	For $30^s.20$ read $31^s.20$.
87	14°.2231	α	For $13^s.64$ read $12^s.64$ (prob.).
87	14°.2326	δ	For 50′ read 40′, in the last line.
91	15°.488	δ	For 37′ read 36′.
91	15°.532	δ	About 15″ too great (2 obs. Ham. Coll.).
91	15°.2175	α	For $35^s.09$ read $36^s.09$ (prob.).
91	+15°.2185	δ	For 44′ read 42′ (?).
94	16°.394	—	Never seen, though often looked for. As it is also in Durchm., it may be a variable. A star was observed, however, in $+3^s$ and +10′ 0″.
99	17°.1818	α	For $10^m\ 56^s.65$ read $11^m\ 2^s.65$ (—the Zones of Vienna and Ham. Coll. agreeing.)
102	18°.1043	α	In the 2d observation read $18^s.66$ (—is W. $5^h.1664$–5.)
105 109	19°.438 $20°.483^a$	α	For $34^s.39$ read $33^s.39$ (W. 2^h 1210 is in agreement with Ham. Coll. Z.)
108	19°.4699	δ	For 15″.1 read 5″.1.
115	21°.1390	α	For 30^m read 40^m.
125	23°.1542	δ	For 42′ read 52′.
130	24°.694	δ	For 3′ read 1′.
131	24°.1985	α	For $45^s.16$ read $46^s.16$ (?)
131	24°.1989	α	For $57^s.29$ read $58^s.29$ (?).
333	$22^h\ 29^m\ 23^s.70$	H	For $23^s.70$ read $24^s.70$.
342	$5^h.116$	α	For 40^m read 49^m (misprint.)
353	13 .55	α	For $57^s.29$ read $58^s.29$.
363	20 .49	δ	For 31° 0′ read 30° 58′.
364	21 .46	δ	For 17′ read 20′ (star is γ Capricorni.)
364	21 .52	δ	For 47′ read 48′ (star is δ Capricorni.)
364	22 .6	α	For $29^s.73$ has LL. 43296: $30^s.62$; Lam. 841: $30^s.99$: R. 9915: $29^s.55$; Wash. Z. Mur. 203.19: $28^s.74$.

III.—*Corrigenda in Weisse's Catalogue of Bessel's Zones between* −15° *and* +15° *of declination.*

NOTE.—The following list contains only the corrections found additional to the many errors known before.

Star.	Col.	
$0^h.47$	δ	For 38′ read 37′.
417	α	Is from 1^s to 2^s too great.
995	α	Is more than 1^s too small.
$1^h.132$	α	For $46^s.33$ read $49^s.33$ (about).
141	δ	7′ read 27′.
210	α	$10^s.10$ read $9^s.10$.
218	α	$38^s.51$ read $39^s.51$.
270	δ	18′ read 16′.
452	α	Is rather more than 1^s too small.
$2^h.22$	α	For $24^s.87$ read $34^s.87$.
142	δ	37′ read 39′.
224	α	$11^s.83$ read $17^s.83$ (about).
$4^h.613$	α	$50^s.09$ read $59^s.09$.
$9^h.478$	α	$48^s.16$ read $46^s.16$.
$10^h.176$	δ	40′ read 39′.
364	δ	8′ read 10′.
381	δ	43′ read 42′.
412	δ	Is about 50″ too large.
438	α	Precession ought to be 3″.172.
438	δ	For 13′ read 12′.
705	α	$15^s.64$ read $25^s.64$.
735	δ	+ read —.
889	α	$31^s.12$ read $30^s.12$ (?).
908	α	$41^s.33$ read $40^s.33$ (?).
927	δ	4° read 3°.
1023	α	Is 1^s too great.
$11^h.273$	δ	For 22′ read 32′.
555	δ	— read +.
560	α	$44^s.76$ read $34^s.76$.
647	α	Seems to be 1^s too small.
667	α	Is rather more than 1^s too large.
687	δ	For 51′ read 50′.
769	δ	For 49′ read 50′.
842	δ	2° read 1°.
847	δ	+ read — (is = LL. 22502).
885	α	Is from 5^s to 6^s too large.
901	α	For 51^m read 50^m.
943	α	Seems about 15^s too large.
$12^h.109$	δ	For 32′ read 33′.
124	α	$13^s.00$ read $12^s.00$.
153	α	$56^s.21$ read $55^s.21$ (? perhaps prop. motion).
181	δ	58′ read 57′.
232	α	Is, perhaps, 1^s too small.
414	α	Is from 1^s to 2^s too small.
438	δ	For 16′ read 15′.
516	α	Is about 2^s too small.
551	α	Is 3^s too great.
567	δ	For 30′ read 32′ (probably).
709	α	Ought to be 1^s smaller.
799	α	For $29^s.90$ read $39^s.90$.
813	δ	32′ read 33′.
939	α	Should be 1^s smaller.
$13^h.242$	δ	For 15° read 14°.
444	α	Seems to be 1^s too large.
595	δ	Requires a correction of about −30″.
838	α	Is 1^s too large (?).
931	α	Probably is to be corrected so that the star becomes identical with 942.
$14^h.49$	δ	For 45′ read 46′.
224	δ	32′ read 33′.
296	δ	11° read 10°.
359	δ	17′ read 19′.
$15^h.355$	α	Is 1^s too great.

III.—*Corrigenda in Weisse's Catalogue of Bessel's Zones, &c.*—Continued.

Star.	Col.	
$15^h.704$	α	For $10^s.17$ read $11^s.17$.
$16^h.312$	α	15^m read 16^m.
888	δ	27′ read 29′.
$18^h.959$	α	37^s read 47^s.
$20^h.23$	α	$20^h\ 0^m$ read $19^h\ 59^m$ (star identical with $19^h.1538$).
41	α	1^m read 0^m (star identical with 19).
1134	α	Is about 3^s too great.
1157	α	For $50^s.95$ read $60^s.95$; precession, 3″.342.
1489	α	$53^s.78$ read $55^s.78$.
1498	α	$22^s.57$ read $12^s.57$.
$21^h.366$	δ	22′ read 32′.
656	δ	42′ read 45′.
665	—	Is not in the skies; but by correcting the right-ascension by -25^s the star will be in the same position as 656 (—the latter corrected in declination as stated here before).
813	δ	For 59′ read 57′.
1150	α	$37^s.81$ read $27^s.81$ (??)
$22^h.228$	—	No star found in this position, nor any plausible correction.
272	α	For 12^m read 13^m (star identical with 295).
313	δ	9′ read 11′.
$22^h.438$	α	For $57^s.48$ read $47^s.48$.
562	δ	[illegible] read —.
704	—	No star seen in this position.
752	δ	For 3′ read 4′.
902	δ	52′ read 49′.
1076	—	There is no star in this position. A correction in right-ascension of -19^s would lead upon 1068, one in declination of +10′ upon a star 9 magnitude observed in Ham. Coll. Zones.
1074	α	For $9^s.43$ read $19^s.43$.
1211	δ	30′ read 31′ (?).
1240	α	$2^s.71$ read $1^s.71$.
1241	δ	31′ read 32′ (?). 1^s = Lam. 4015.
$23^h.713$	α	$21^s.56$ read $22^s.56$.
718	α	33^m read 34^m.
723	α	$3^s.29$ read $4^s.29$.
834	α	39^m read 38^m (same star as 808).
837	α	39^m read 38^m (same star as 811).
905	δ	35′ read 36′.
958	δ	40′ read 41′.
964	δ	11′ read 12′.
1099	α	Is 1^s too great.
1154	δ	For 9′ read 7′.

Furthermore, for the stars $11^h.848$, 849, 850, 855, 858, the precession in right-ascension ought to be 3^s instead of 2^s, and for the stars $11^h.723$ and 724 the precession in declination ought to be 19″.

IV.—*Corrigenda in Weisse's Catalogue of Bessel's Zones between +15° and 45° of declination.*

Star.	Col.	
$1^h.1153$	α	Is 1^s too great.
$3^h.417$	α	For $29^s.44$ read $19^s.44$.
1106	δ	59′ read 58′.
$4^h.731$	δ	50′ read 55′.
$5^h.1266$	—	There is no star in the position given, which ought to be corrected either by reading the right-ascension 35^m for 36^m or by reading the declination 53′ for 13′.
1269	δ	For 15′ read 5′.
2032	δ	10′ read 6′.
$6^h.265$	δ	4′ read 3′.
935	δ	Is 30″ too large (—star = Y. 2682).
965	α	For 30^m read 29^m (same star as 927 and 929).
$8^h.210$	δ	For 18′ read 8′.
1122	α	For $41^s.10$ read $21^s.10$.
$9^h.833$	α	40^m read 41^m.
$12^h.587$	α	$5^s.83$ read $15^s.83$.
$16^h.970$	α	$0^s.65$ read $10^s.65$ (same star as 976).
$17^h.307$	α	$12^s.88$ read $7^s.88$.
$18^h.763$	α	$3^s.78$ read $5^s.78$ (?)
$19^h.52$	α	$31^s.21$ read $1^s.21$.
1473	α	44^m read 45^m.
1537	δ	Is from 30″ to 40″ too large (two observations in Ham. Coll. Zones).
1683	α	For $13^s.47$ read $23^s.47$.
$20^h.401$	α	$42^s.51$ read $43^s.51$ (probably).
$22^h.162$	α	$24^s.09$ read $23^s.09$.

V.—*Corrigenda in Rümker's Catalogue of* 12000 *stars.*

No. R.	Col.	
1637	α	For $36^s.00$ read $38^s.00$.
1649	α	Is about 7^s too large.
1714	δ	For 17′ read 16′.
1859	—	Does not exist. Decl. for 23° prob. 33°.
1897	—	Not seen. Decl. for 23° prob. 33°.
1997	δ	For 52′ read 22′.
2010	α	42^m read 44^m.
2636	δ	20° read 10° (prob., and the star 49 Cancri).
2762	δ	Somewhat uncertain.
3087	α	For $4^m\ 36^s$ read $5^m\ 26^s$ (about).
3130	—	Position erroneous; perhaps to be corrected by $+5^s$ in AR. and +3′ in Decl.
3157	δ	For 35° read 36° (the star is 26 Leonis min.).
3219	δ	16° 1′ read 15° 59′ (?).
3332	δ	15′ read 14′.
3778	δ	24′ read 23′.
5332	δ	29″.5 read 59″.5.
9218	δ	15′ read 5′ (prob., as thus we come upon a star of 9.10^m, while in the Cat. position there is none to be seen).
9248	δ	11° read 12°.
9254	δ	For 11° read 12°.
9314	—	No star here; correct α by $+7^s$ (=No. 9315).

V.—*Corrigenda in Rümker's Catalogue of* 12000 *stars*—Continued.

No. R.	Col.	
9395	α	For 55^s read 56^s (prob.).
9965	α	$38^s.75$ read $48^s.75$ (about).
10307	α	22^m read 20^m (the star is 58 Aquarii).
10427	α	44^s read 45^s.
10439	α	20^s read 21^s.
10492	δ	24′ read 22′ (prob.).
10559	δ	— read + (?).
11789	δ	13′ read 12′.
11894	δ	29′ read 30′.

Nachtrag $6^h\ 59^m\ 41^s$ for 18′ in Decl. read 17′; the star is the variable R Canis min.

Nachtrag $12^h\ 13^m\ 12^s$ prob. is too small in AR. by $1^m\ 7^s$, and the star identical with Schj. 4459.

No star was seen in the position of No. 4338; but in Ham. Coll. Z. a star was observed 4^s greater in AR, and 1′ 35″ farther north, which might be the star of Rümker.

Not found are the following stars of this Catalogue, viz: 1399, 3607, 3610, 3698, 9926, 10006, 10072, 10109, 10516, 11933, for which a revision of the original manuscripts would be desirable.

VI.—*Corrigenda in Rümker's Catalogue, new series.*

No.	Col.		No.	Col.	
470	α	About 45^s too small.	834	δ	For 34′ read 33′.
534	δ	For 1′ read 11′.	1620	α	$3^s.914$ read $3^s.194$ in precession.
564	δ	8° read 7°.	2091	δ	About 10″ too small (LL. 7581 and 2 obs. Ham. Coll. Z.).
638	α	15^m read 16^m.	2633	δ	For 13 read 3 (star is 33 n′Orionis).
663	δ	59″.4 read 19″.4 (3 obs. Ham. Coll. Z.).			
732	—	There is no star in this place.			

VII.—*Corrigenda in Schjellerup's Catalogue.*

NOTE.—Several of the corrigenda contained in the following list may have been indicated before.

No.	Col.		No.	Col.	
568	α	For $54^s.26$ read $55^s.26$ (?).	9234	δ	For 47′ read 48′.
568	δ	75″ read 45″ (misprint).	9307	δ	35″.9 read 25″.9.
572	δ	45′ read 43′ (?).	9447	α	Probably to be corrected by -20^s, since no star is found in the place of the Catalogue.
805	—	W. 787 read W. 788.			
3780	α	$38^s.08$ read $39^s.08$.			
4261	δ	56′ read 55′.			As synonyms may be inserted the following:
4424	δ	9° 0′ read 8° 59′.	3474	2	LL. 18585.
4588	δ	13′ read 14′.	4065	3	W. 50.
8467	α	55^m read 56^m (misprint).	7982	3	W. 316.
8706	α	$23^m\ 21^s.15$ read $22^m\ 51^s.15$.	8832–3	3	W. 891 (corrected by $+1^m$).
9027	α	$54^s.82$ read $55^s.82$.			

VIII.—*Corrigenda in Baily's Catalogue of Lalande's Zones.*

NOTE.—The following few mistakes have escaped the scrupulous investigation of Argelander, and are not noted in the extensive list published in Vol. VII of the Bonn Observations.

No.	Col.		No.	Col.	
5229	α	For 38^m read 37^m.	35071	α	For $17^s.90$ read $7^s.90$.
30685	P. D.	23′ read 24′.	45064	P. D.	36′ read 31′.
30784	P. D.	39′ read 41′.	45189	P. D.	11′ read 12′.

IX.—*Corrigenda in Yarnall's Catalogue, second edition* (1878).

NOTE.—This catalogue in greater part is derived from observations with the Transit instrument for the right ascensions and with the Mural circle for the declinations. Very often, however, the precaution was neglected to read the second co-ordinate with sufficient approximation for identifying the star beyond question. Therefore in composing the catalogue not seldom wrong combinations of right ascension and declination were made, giving rise to spurious stars, which of course are found among the many anonymous of which the catalogue abounds. To sift these, a comparison with other catalogues was made by me. Professor *Millosevich* recently has published (*Annali della Meteorologia Italiana*, Parte III, 1881) a complete investigation of all the anonymous in Yarnall's Catalogue that come within the limits of the *Durchmusterung*. For these, therefore, I have suppressed my notes, except where I differ from my distinguished friend in the conclusions arrived at, or where a more detailed elucidation, by going back to the *Washington Observations*, seemed necessary for confirming some of his very happy conjectures. Below I append a supplemental list of Yarnall's anonymous, the positions of which I found secured by other catalogues.

Cat. No.	Col.	
119	—	The declination should be *south* (not *north*, as seen from Wash. Obs., 1861), and is that of LL. 310=Dm.—0°.37. But the right ascension belongs to another star, viz. Dm.+0°.29, which is found also in the Harvard Zones.
150	—	The declination belongs to Y. 159; the right ascension is that of W_2.0^h.369, of which the declination, however, was not observed at Washington.
198	—	The magnitudes as well as the declinations of these two stars should be interchanged, the preceding star being the northern and smaller one.
199	—	
329	α	For 33^m read 34^m (—the same mistake also in Weisse's catalogue, $W_1$0^h, 572).
341	—	There is no star in this place; there must be some mistake in the transit in 1865, from which the right ascension is rightly reduced.
378	α	For 39^m 26^s.12 read 40^m 27^s.14 (—error in Wash. Obs., 1865, p. 111, No. 35). The star is identical with Y. 388.
590	α	For 1^m read 0^m. It is the right ascension of Dm.+3°.161; but the declination belongs to another star, viz. R_2.538.
664	δ	The reduction of the declination from Wash. Obs., 1871, App. II. p. 61, is erroneous. It should be —1° 10′ 58″.6, and belongs still to Y. 668 (43 Ceti): the declination of Y. 664 has not been observed at Washington.
848	—	Is to be canceled, originating from the combination of several errors in the Wash. Obs., 1861 (pag. 163, 1st star, and pag. 224, No. 47). When these are corrected, the right ascension for 1860 comes 1^h 42^m 48^s.25, which is that of Y. 854, and the declination +13° 39′ 9″.9, which is that of Y. 852.
937	α	For 55^m read 56^m (—error in Wash. Obs., 1868, p. 359).
962	δ	Is erroneous. Of the two observations one was made in 1874, and gives +38° 58′ 59″.6, agreeing with W_2.4^h.1168. Of the other, made in 1852, the originals have never been published.
1342	α	For 4^m read 5^m (—error in Wash. Obs., 1871, App. II. p. 16). The star is also Arg. +8°.474.
1776	—	Does not exist. The declination probably was wrongly noted *north*, and should be *south*. This makes the right ascension change to 38^s.24, and hence the star identical with Y. 1777.
1894	—	Has in the 1st edition the number 1896.
2098	α	Mean year: for 31.6 read 63.1 (—error carried over from 1st edition). Also the "number of obs." prob. should be 1 instead of 2,—this one observation being made on Feb. 2, 1863, the result of which shows exactly the same figures as the right ascension in Catalogue.
2133	δ	For 32′ 47″.4 read 29′ 53″.1 (—error in Wash. Obs., 1870). The star hence identical with Y. 2134.
2139	α	The reduction to mean equinox in Wash. Obs., 1862, p. 8, is erroneous, and the star identical with Y. 2135.
2275	—	Has in 1st edition the number 2270.
2311	α	For 28^m read 29^m (—as seen in Wash. Obs., 1869, p. 260).
2362	α	" 40^s.35 read 38^s.35 (—error in Wash Obs., 1877, p. 103, No. 2).
2373	α	" 2^s.885 read 2^s.285 in precession.
2594	—	Both magnitudes and right ascension of these two stars seem to be interchanged.
2595	—	
2704	—	There is no star in that position. But changing the approximately noted declination from +23° 49′ into 24° 1′, the star is W_2.6^h.1080.
2779	δ	For 41″.1 read 12″.8. Although the 2 observations with the Mural Circle agree, the declination must be about 30″ farther north. Probably at both times an error of 1 revolution=31″.34 was committed.
2796	δ	Belongs still to Y. 2798; the approximate right ascension erroneous.
2798	δ	For 47′ read 46′ (—as seen from Wash. Obs.).
2876	δ	" 11′ read 12′.
2886	—	Is identical with No. 2889 (also Ö. Arg. 6317).
2952	δ	For 13′ 56″.4 read 14′ 13″.7. In Wash. Obs. 1870, pag. 125, No. 19 is put *south*, and hence also the reduction to mean equinox made erroneous. Yarnall has corrected the sign of the declination, but left the reduction unchanged. The star appears now to be identical with No. 2953.
3572	δ	" 57′ read 55′.
3718	α	" 42^m read 43^m.
3729	—	Is No. 3734 in 1st edition.
3730	—	" 3729 in 1st edition.
3731	—	" 3730 in 1st edition.
3732	—	" 3731 in 1st edition.
3733	—	" 3732 in 1st edition.
3734	—	" 3733 in 1st edition.
3787	α	For 50^m 50^s.87 read 51^m 0^s.87 (—error in Wash. Obs., 1862, p. 31, No. 13), so that this right ascension belongs still to No. 3792. The declination, however, is probably that of Arg. +23°.2024, of which the right ascension was not observed at Washington.
3964	δ	— read + (misprint).
4111	δ	The approx. declination should be +18° 5′ (s. Wash. Obs., 1877), and the star is identical with Arg. +18°.2278.
4114	—	There is no star here. The right ascension for 44^m should be 45^m probably, and the Circle reading was either 5′ or 10′ wrong, so that the star is identical either with Y. 4117 or with Y. 4118.

IX.—*Corrigenda in Yarnall's Catalogue, second edition* (1878)—Continued.

Cat. No.	Col.	
4177	δ	For 32′ read 31′.
4285	α	$20^{s}.74$ read $18^{s}.74$ (—error in Wash. Obs., 1869, p. 269).
4598	δ	+ read —.
4598	α	$3^{s}.158$ read $2^{s}.986$ in precession.
4693	—	Right ascension and declination are of two different stars.
4879	—	α and δ belong to two different stars, and are wrongly combined. The right ascension is that of Lam. 1181, the declination probably that of a star that was observed also by Wash. Equatorial, A. N., No. 873. In Ham. Coll. Zones were observed: $11^{h}\ 32^{m}\ 38^{s}$; —5° 14′ 21″ (Lam. 1181) 33 1 20 3 (Wash. Equ.) 33 18 19 48
5269	—	There seems to be no good authority for calling this star *Var.* It is 6.7 magnitude.
5459	—	For g Virginis read 49 Virg. (s. Argelander in Bonn Obs., VII, p. 404).
5831	δ	25° read 23° (as it is in Wash. Obs., 1849).
5831	α	$2^{s}.749$ read $2^{s}.777$ in precession.
6347	δ	42′ read 38′ (s. Wash. Obs., 1848). The star is R. 5070.
6538	δ	23′ read 21′.
6832	δ	48′ read 47′ (s. Wash. Obs., 1871, App. II, p. 78). The star is 29 h Herculis.
6999	α	$3^{s}.716$ read $2^{s}.716$ in precession.
7016	δ	58′ 59″.9 read 59′ 31″.5. The correction of 1 rev. =31″.6 is demanded by 5 observations in the Wash. Zones.
7022	δ	Is over 5′ too far south. The star is Ö. Arg. 16153.
7027	α	Prob. 30^{s} too great, and the star is identical with No. 7021.
7252		For $2^{s}.571$ read $3^{s}.571$ in precession.
7355		$46^{s}.83$ read $50^{s}.53$. The right ascension $46^{s}.83$ probably was Ö. Arg. 17088, the declination of which was not observed at the Mural Circle.
7356	α	Belongs to the preceding star, and is here to be canceled. The right ascension of Ö. Arg. 17094 was not observed.
7582	δ	For 6″.8 read 16″.8 (—error of Cat.; Wash. Obs. correct).
7965	δ	26′ read 27′ (s. Wash. Obs., 1845, p. 267; by oversight the star is omitted in the special catalogue on p. 273).
8047	δ	38′ read 37′ (—right in Wash Obs.).
8328	—	There is no star to be seen in this place. The right ascension agrees with O.-Arg. 19616, the declination (to about 20″) with O.-Arg. 19500-2.
8486	δ	For 8′ read 10′.
8490	δ	8′ read 9′.
8665	α	$8^{s}.36$ read $9^{s}.36$ (LL., O.-Arg., Lam. agree).
8670″	—	Seems to be identical with 8670‴.
8768	δ	For — read + (misprint).
8868	—	α and δ are wrongly combined; the right ascension is that of R. 8288=O.-Arg. 20475, the declination that of R. 8291 = O.-Arg. 20493.
9067	α	For 40^{m} read 41^{m} prob., and the star = Y. 9078.
9218	Mag.	8.0 should be rather 4.0—error carried over from the 1st edition.
9382	—	No star seen in this place. By assuming in Wash. Obs., 1868, p. 298, No. 38, the micrometer reading 32 rev. instead of 39 rev., the declination would result —23° 49′ 11″.1, and the star become identical with No. 9381.
9521	—	In the Wash. Obs., 1846, p. 439, No. 520, are two errors: for $38^{m}\ 46^{s}.83$ and 50′ 40″.69 ought to be read $33^{m}\ 46^{s}.83$ and 50′ 14″.69. This makes the position for 1860: $21^{h}\ 34^{m}\ 11^{s}.14$; + 38° 52′ 55″.9; which is therefore another observation of Y. 9481.
9633	α	This right ascension belongs still to η Piscis Aust.; that of Lac. 8989 has not been observed at Washington.
9666	α	For $34^{s}.84$ read $37^{s}.84$ (perhaps typographical error).
9678	α	58^{m} read 57^{m}—error of reduction from Wash. Obs., 1869, where, on p. 289, the right ascension is given correctly. The star is identical with Y. 9669.
9975	δ	22′ read 21′. The error exists already in Wash. Obs., 1871, App. II., p. 96; but the originals for 1855 are not published.
10304	α	15^{m} read 16^{m} (s. Wash. Obs., 1875).
10340	δ	13′ 56″.7 read 9′ 6″.0—error originated from erroneous *refraction* in Wash. Obs., 1869, p. 221, No. 44; for 5′ 11″.8 read 0′ 31″.0. The star is identical with No. 10339.
10342	—	Is still an other observation of the star just mentioned. In Wash. Obs., 1846, p. 18, No. 12, the instrumental correction is erroneously computed; for $+0^{s}.81$ it should be $-1^{s}.35$. Then the mean of the 2 observations made in 1846 gives the right ascension of Y. 10339: $43^{s}.14$ (2) instead of $42^{s}.93$ (1). The declination (Wash. Obs., 1859, p. 218, No. 53) is probably in error by 1 revolution =31″.2, and should be 5″.1 instead of 36″.3.
10495′	δ	For 14′ read 1′ (misprint). The star is Dm. +67°.1562.
10577	—	Is not O.-Arg. 23156, but 23141 (corrected by $+1^{m}$).

Here follows a list of "Anonymous" identified in other catalogues. Arg. VI means the 6th volume of the Bonn Observations; Lamont's number refers in each case to the particular catalogue for the corresponding declination.

No. Yarnall.	Catalogue.
745	Arg. VI. 1^h.37.
914	Lam. 291.
928	La Caille 600.
945	Arg. VI. 1.92.
1107	Arg. VI. 2.37.
1313	O.-Arg. 2007 (δ corr.).
1480	Arg. VI. 3.57.
1695	Arg VI. 3.96.
1738	Arg. VI. 3.113.
1888	Arg. VI. 4.53.
1970	Arg. VI. 4.99.
2060	Arg. VI. 4.108.
2115	Arg. VI. 4.142.
2193	O.-Arg. 3742.
2202	Arg. VI. 5.14.
2225	O.-Arg. 3846 (prob.).
2297	Lam. 59.
2464	Lam. 160.
2571	Arg. VI. 6.20.
2572	Arg. VI. 6.2[illegible].
2573	Arg. VI. 6.22.
2635′	O.-Arg. 5144.
2672	Arg. VI. 6.102.
2716	Lam. 357.
2785	Lam. 393.
2790	Arg. VI. 6.171.
2809	Arg. VI. 6.1[illegible]4.
2927	O.-Arg. 6484–5.
2943′	O.-Arg. 6536.
3045	Arg. VI. 7.22.
3180	O.-Arg. 7449.
3255	O.-Arg. 7740–1.
3432	Arg. VI. 8.63.
3434	Arg. VI. 8.66.
3451	La Caille 3374.
3456	O.-Arg. 8697.
3476	Arg. VI. 8.81.
3652	Lam. 460.
3667′	O.-Arg. 8870 (prob.).
4092′	W. 9^h.910.
4297	Lam. 859.
4357	Lam. 894.
4448	Lam. 775.
4543	Schj. 3972.
4566	Lam. 3134.
4822	Lam. 3373.
4838	Lam. 1163.
5034	Lam. 1096.
5035	Lam. 1097.
5104	O.-Arg. 12021–2.
5333′	O.-Arg. 12425.
5403	W. 12^h.859.
5607	Arg. VI. 13^h. 48.
5860	O.-Arg. 13447–8.
5948′	LL. 26307.
5984	Lam. 1702.
6024′	O.-Arg. 13758.
6179	Arg. VI. 14.107.
6204	O.-Arg. 14250.
6257	O.-Arg. 14362 (? corr. $+20^s$).
6373	Arg. VI. 15.32.
6377	O. Arg. 14614.
6481	Arg VI. 15.53.
6502	Arg. VI. 15.63.
6532	Lam. 1911.
6552	Arg. VI. 15.75.
6555	LL. 28913.
6696	Arg. VI. 16.6.
6937	O.-Arg. 15989 (corr. +10′).
6953′	O.-Arg. 16019.
6962	Arg. VI. 16.61.
6964	O.-Arg. 16035.
6964′	O. Arg. 16037–8.
6976′	O.-Arg. 16062.
6980′	O.-Arg. 16067.
7013	O.-Arg. 16133–4.
7064	Arg. VI. 16.92.
7198	Arg. VI. 17.31.
7199	Arg. VI. 17.33.
7201	Arg. VI. 17.36.
7317	Arg. VI. 17.68.
7421′	O.-Arg. 17242.
7428	O.-Arg. 17260.
7429′	O.-Arg. 17262.
7470	Arg. VI. 17.102.
7589	Arg VI. 17.118.
7600	Arg. VI. 17.122.
7694	Arg. VI. 18.16.
7713	Lam. 535.
7719	Lam. 29.
7777	Lam. 58.
7815″	Lam. 41 (corr. -1^s).
7878	Arg. VI. 18.97.
7906	Lam. 60.
7915	Lam. 63.
7919	LL. 34650.
7719′	LL. 34661–3.
8035	Lam. 750.
8051	Lam. 2951 (?).
8054	W. 18^h.1293
8090	Lam. 3003.
8091	Lam. 3007.
8114	O.-Arg. 19058.
8122	Lam. 797.
8157	Lam. 2688.
8169	Lam. 3077.
8174	Lam. 3083.
8178	Lam. 3091.
8184	Lam. 109.
8248	Lam. 875.
8309	O.-Arg. 19551.
8341	Arg. VI. 19.57.
8362	Lam. 150.
8372	O.-Arg. 19735.
8406	Lam. 161 (corr. -10^s).
8408′	LL. 37162.
8417′	LL. 37207.
8426	LL. 37236.
8434	Lam. 358.
8439	Lam. 169.
8441	LL. 37290.
8470	Arg. VI. 19.79.
8493	Lam. 176.
8511	Lam. 3012.
8524′	LL. 37791.
8538	Lam. 401.
8574	Lam. 411.
8585	Lam. 1046.
8624	Lam. 426.
8677′	O. Arg. 20215 (corr.).
8688	Lam. 1097.
8700	Lam. 1102.
8708	Lam. 3120.
8718	Lam. 3126.
8725	Lam. 3130.
8735	Lam. 1118.
8753	Lam. 477.
8761	Lam. 479.
8828	Lam. 502.
8849	Lam. 515.
8855	Lam. 517.
8868	O.-Arg. 20508.
8900	Lam. 1205.
8922	Lam. 7632.
8930	Lam. 7645.
9020	Lam. 595.
9040	Lam. 3374.
9048	Lam. 317.
9051	Lam. 318.
9093′	O.-Arg. 20902.
9130	Lam. 639.
9134′	O.-Arg. 20966.
9149	Lam. 7930.
9152	Lam. 3452.
9157	Lam. 340 (corr. +10′).
9174	LL. 40599.
9228	Lam. 362.
9246′	LL. 41021.
9260	Lam. 3546.
9261′	LL. 41100.
9294	Lam. 702.
9322	Lam. 714.
9334	Groombr. 3445.
9363	Lam. 727.
9380	O.-Arg. 21434 (corr. -1^m).
9381	O.-Arg. 21436 (corr. -1^m).
9399′	O.-Arg. 21447.
9401	Lam. 740.
9404′	O.-Arg. 21466.
9417	Lam. 4317.
9429	Lam. 1426.
9439	Lam. 4328.
9518	Lam. 3752.
9529	Lam. 1464.
9542	Lam. 8544.
9549	Lam. 8551.
9556	Lam. 8563.
9558	R. Nachtrag 132.
9567	Lam. 1472.
9675	O.-Arg. 21870.
9735	R. 9903.
9809′	W. 22^h.295.
9848	Arg. VI. 20.54.
9850	Arg. VI. 20.55.
9907	O.-Arg. 22220 (corr. $+1^m$).
9928	Lam. 3931.
9948	Lam. 514.
9993	Schj. 9302.
10036	Lam. 4662.
10129	LL. 25028 (?).
10622	O.-Arg. 23208.

X.—*Corrigenda in the Glasgow Catalogue of 6415 stars.*

No.	Col.	
1640	N. P. D.	For 5′ .94 read 59′ .48 (misprint).
3289	α	52^{m} read 53^{m}. The star is rightly identified with W. 837; it is also Schj. 4685, and Dm. + 10° .2496; so that there remains no doubt about the correction. This changes also the precessions a little.
4051	α	20^{m} read 19^{m}. This, together with the correction of north polar distance indicated in the *errata* on page lxvii, makes the star identical with No. 4044, which is LL. 29924, W. 362, and Dm. + 8° .3194. There is no star in the uncorrected place.

In the precessions some mistakes, besides those pointed out in the *Errata*, have been detected.

No.	Read precession in right ascension.
708	2″ 9930
982	2 .9729
1274	2 .9627
1947	3 .5306
3289	3 .0166 (for correction in position).
3300	3 .0207
4067	3 .0237
4258	2 .8177
4261	2 .7817
4262	3 .0202
4363	2 .4641
4476	3 .2680
4558	2 .8996
4953	2 .8345
5079	2 .8158
5169	2 .3320
5522	3 1923 (for correction in north polar distance).
5912	3 .1072
6095	2 .9744

No.	Read precession in north polar distance.
931	10 .621
2375	11 .548
2577	16 .786
3143	20 .023
3289	19 .516
3484	17 .669
3485	17 .667
4694	5 .106
5390	14 .651
5439	15 .107

For No. 3955 the "secular variation of precession" should be read + 0 .0142 instead of + 3 .4796 (indicated by Auwers).

XI.—*Corrigenda in the catalogues of the two volumes of "Observations at Santiago de Chile," published by Moesta.*

A. In Volume I (Observations 1853–'55).

No.	Col.	
24	δ	For 119° 51′ 28″ .30 read 122° 51′ 30″ .74.
24	α	30^{s}.38 read 30^{s}.31.
32	δ	59′ read 58′.
51	δ	24′ read 14′.
60	δ	9″.21 read 19″.21.
95	δ	46′ read 47′.
151	δ	46′ read 45′.
152	δ	40′ read 39′.
209	δ	43′ read 53′ (the star is W. 3^{h}.233).
211	δ	37′ read 38′.
219	δ	44′ read 34′.
260	δ	39′ read 41′.
316	α	39^{s}.60 read 9^{s}.60.
341	δ	70° 0′ 2″.20 read 68° 55′ 1″ .94.
341	α	33^{s}.24 read 32^{s}.43 (the star is W. 5^{h} .1344–5).
470	δ	36° 26′ .85 read 35° 2′ .98.
618	α	59^{m} read 58^{m}.
655	δ	For 34′ read 41′.
675	δ	3′ read 2′.
682	α	0^{s}.20 read 1^{s}.20.
743	δ	132° read 133°.
762	δ	23′ read 30′ (is Lac. 7202).
774	α	51^{m} read 50^{m}.
788	δ	58° 35′ .0 read 59° 11′ .0.
813	δ	31′ read 41′.
849	δ	54′ read 33′.
850	δ	7° 43′ .86 read 8° 39′ .97.
888	Name	Lac. 8778 read 8777.
926	δ	120° 17′ 7″.37 read 114° 22′ 4″.42.
926	α	21^{s}.17 read 21^{s}.28.
989	α	43^{s}.56 read 44^{s}.41.
992	α	46^{s}.31 read 47^{s}.19.
994	δ	32′ read 22′.
995	α	53^{s}.12 read 20^{s}.31.

XI.—*Corrigenda in the catalogues of the two volumes, &c.*—Continued.

B. In Volume II (Observations 1856–'60).

No.	Col.		No.	Col.	
236	α	Prec., for $2^s.631$ read $3^s.514$.	1066	δ	For 37 read 38.
236	α	For $33^m58^s.62$ read $34^m2^s.15$ (the star is LL. 6786).	1087	α	$46^m 44^s$ read $47^m 4^s$.
263	α	Prec., for $2^s.378$ read $2^s.271$.	1248	δ	$51'$ read $41'$ (prob.).
263	α	For $41^s.70$ read $41^s.37$.	1249	δ	52 read $32'$.
386	δ	57 read 58.	2022	δ	116 read 146.
425	δ	$30'$ read 29.	2102	δ	101 read 100 (is = LL. 41921).
546	δ	118° read 138°.	2109	α	29^m read 28^m } (the star is W. 21^h.662).
800	α	$37^s.87$ read $32^s.87$.	2109	δ	101 read 100 }
805	α	$28^s.17$ read $29^s.17$.	2257	δ	95 read 96 (is = LL. 45340).

XII.—*Corrigenda in the catalogues of the Geneva Observations from 1842–'49.*

Year.	Page.	Star.	Col.	For—	Year.	Page.	Star.	Col.	For—
1842	69	39 Andromedæ..	δ	41° read 40°.	1844	87	A Tauri........	α	4^h read 3^h.
	77	24 n. Scorpii	α	28^s read 26^s.		90	Pi. $8^h.48$........	δ	11 read $9'$.
	77	42 θ Ophiuchi...	α	$22^s.67$ read $7^s.67$.	1846	91	W. $14^h.1158$....	α	$50^s.73$ read $47^s.73$.
	78	Piazzi 282....	Name	282 read 301.	1847	69	Anon. $8^h 27^m$ ..	δ	15 read 14.
	78	Piazzi 282....	α	$39^s.10$ read $34^s.10$.		72	W. $11^h.687$. ...	α	$9^s.09$ read $11^s.09$.
1843	82	42 Leonis...... .	δ	16° read 15°.		75	An. $10^h 46^m 49^s$..	α	46^m read 45^m.
	84	(1585) Virginis	δ	18 read 17.	1848	97	Anon. $10^h 11^m$ ..	δ	22 read 21.
	84	3 Serpentis	δ	$32'$ read 31.		97	W. $10^h.620$......	α	43^m read 34^m.
	85	6 Serpentis	δ	7 read 17		108	LL. 43294	α	$21^s.63$ read $22^s.63$.
	85	10 Serpentis	α	2^s read 42^s.		100	LL. 43297	α	$27^s.98$ read $28^s.98$.
	87	Pi. $17^h.376$ ...	δ	$19'$ read 9.	1849	111	Pi. $13^h.291$......	δ	57 read 51.
	90	Anon. $20^h 18^m$.	δ	22 read 21.		119	Pi. $21^h.173$......	α	$21^s.40$ read $22^s.71$.
	91–2	Pi. $21^h.173$ ...	δ	48 read 47.					

Supplement to Corrigenda in Yarnall's Catalogue.

No.	Col.		No.	Col.	
2033	α prec.	For $3^s.010$ read $3^s.310$.	6654	α prec.	For 2,531 read 3,531.
2209	δ	+ read —.	7036	α prec.	3,713 read 2,713.
4200	α prec.	2,237 read 3,237.	7562	α prec.	+2,501 read — 2,501.
4678	δ	+ read —.	7609	α prec.	3,064 read 4,064.
4694	α prec.	3,378 read 3,578.	8622	δ	+ read —.
5091[1]	α prec.	28,44 read 2,844.	9442	δ	+ read —.
5526	δ	+ read —.	9550	δ	+ read —.
6451	α prec.	3,148 read 2,148.	9705	δ	+ read —.
6510	α prec.	3,787 read 2,787.	9751	α prec	Read 3,417.
6516	α prec.	3,050 read 2,050.	6651	Name	For Anon. read W. $15^h.1109$.
6563	α prec.	+ 3,561 read — 3,561.			

Additions to be inserted:

O. Arg (South Cat.) 21966: α is 3^s too small (also in Z. 256.87).
W. $1^h.445$: δ is $2'$ too great (?).
W_2. $5^h.1275$: α is 6^s too great.
Schj. 5464: α precession for $3^s.271$ read $3^s.261$.
Y. 1121: δ for 44 read 43 (see Wash. Obs., 1862, p. 317, and 1869, p. 424).
Y. 8851: α for $4^s.67$ read $3^s.67$ (is correct in the Wash. Obs.).
Y. 9338: α precession, for $2^s.496$ read $3^s.496$ (error carried over from 1st edition).

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

TWELFTH MEMOIR.

RATIO OF METER TO YARD.

RATIO OF METER TO YARD.

By C. B. Comstock.

READ APRIL 21, 1885.

Before the close of the work of the Lake Survey, a steel meter then designated R 1876, was sent to the Bureau International des Poids et Mesures, at Sèvres, France, for comparisons with the standards of that Bureau. It has recently been returned with values for its length and for its coefficient of dilatation. As it had previously been compared in the Lake Survey office with the Clarke yard A, a yard which had been carefully compared by Colonel Clarke with the standard yard Y_{55} of the Ordnance Survey, the comparisons give a value for the ratio between the yard and meter.

The value for that ratio which for some years was supposed most exact, is given by Colonel Clarke in his comparisons of standards of length, and was derived from comparisons with several closely agreeing toises, dependent for their length on the toise of Peru. As the meter was legally defined to be 443.296 lines of the toise of Peru, Colonel Clarke, from this definition, found the meter equal to 1.09362311 yards, or 39.370432 English inches.

The meter of the archives was intended to satisfy this definition, but when adopted as a standard the ideal meter became the length at 0° C of the bar of platinum called the meter of archives, and no longer depended on the toise for its length.

In recent years it has been known that the ratio obtained by Colonel Clarke needed correction, and as a value for it, which cannot be largely in error, can be obtained from the Lake Survey comparisons already mentioned, I have thought the result might be of interest to the Academy. The details have been communicated to the Chief of Engineers and will probably soon be published.

The comparisons made in the Lake Survey office of the Clarke yard A with the metre R 1876, and by Colonel Clarke with the Ordnance Survey standard Y_{55}, may be found in the Report on the Primary Triangulation of the United States Lake Survey. From those comparisons and from the value of Y_{55} in terms of the English prototype yard No. 1, there results:

$$R\ 1876 = 1.^{y}09388063 \text{ at } 57.^{\circ}92\ F.$$

The errors which enter this value are those in the value of Y_{55} in terms of the English prototype yard No. 1; those in the value of Clarke yard A in terms of Y_{55}; and those in the value of Meter R 1876 in terms of Clarke yard A. As to the probable error in the value of Y_{55}, given by Colonel Clarke as $0.^{y}9999996$ at 62° F. little is known, and Colonel Clarke's "Comparisons of Standards of Length" does not indicate that Y_{55} has been compared with the prototype No. 1 since 1853. At the time of its construction the value given by Mr. Sheepshanks for Y_{55} at 62° F. was $1.^{y}00000043$, differing about one millionth of a yard from the value given above by Colonel Clarke, which value results from intercomparisons by him of five standards under the assumption that the mean length of these standards had, in 1864, the same relation to the yard as that found by Mr. Sheepshanks in 1853. Moreover, there is a possibility that the prototype No. I has changed length by a small quantity.

The value of Clarke yard A in terms of Y_{55} is known with accuracy, probably to within less

than a millionth part. The value of R 1876 in terms of Clarke yard A was very carefully determined. But the comparison of a line meter with an end measure yard is one involving many operations, and of great delicacy. While the computed probable error in the value of R 1876 in terms of Clarke yard A at 57° 92 F. was less than $\frac{1}{2000000}$ part, the real error may be considerably greater.

Considering these sources of error or uncertainty, it would not be safe to assign a probable error to the value of R 1876, given above, less than from $\frac{1}{300000}$ to $\frac{1}{500000}$ part of its length.

The value of R 1876 in terms of the English yard having now been given, its value in terms of the meter, will next be considered.

The Bureau International des Poids et Mesures in their comparisons of R 1876 have designated it as U. S. (Repsold,) and give for its length at 0° C.

$$\text{U. S.}_{\circ}=1000097^{\mu}.81.$$

and for its coefficient of dilatation between about 0° and 36° C. $\alpha_t=0.000010563\pm0.000000011$. The details of the work may be found in Tome III, Travaux et Memoires Bureau International des Poids et Mesures.

This value of U. S. (Repsold) results from its comparisons with a meter of the International Bureau known as type II. The value of type II has been determined by the International Bureau in terms of another platinum-iridium meter designated as I_2, with the highest accuracy.

I_2 has been directly compared with the mètre des archives and the committee in adopting provisorily as a unit of length $I_2-6^{\mu}=1$ meter at 0° C. state that this value can only be changed by some tenths of a micron when the prototype meter is finally adopted.

As the probable error in the difference of U. S. (Repsold,) and type II is but a few tenths of a micron, that of type II and I_2 less than one-tenth, and that of I_2, with reference to the prototype yet to be adopted, only some tenths of a micron, it will be seen that the value of U. S. (Repsold)=R 1876, given above, is probably not in error by one micron.

From the value of U. S. (Repsold) at 0° C. and from its mean coefficient of dilatation given by the International Bureau, its length at 57° 92 F. is U. S. (Repsold)$=1^{m}.0002499$. Comparing this with its value at the same temperature in terms of the English yard previously given, there results:

$$\frac{\text{meter}}{\text{yard}}=1.093607, \text{ or meter}=39^{\text{in}}.3699$$

In the Primary Triangulation of the Lake Survey a value for U. S. (Repsold) is given, furnished me by Professor Foerster of the Standards Bureau at Berlin. The value is R 1876 $=1^{m}.00008618$ at 0° C. a value $11.''6$ less than the one now given by the International Bureau. The value given by Professor Foerster depended on the value of the meter type I of the International Bureau, for which he used the value at 0° C., type $I=1^{m}.0000676$. This was doubtless the best value then known to the International Bureau, and was derived from indirect comparisons with the mètre des archives. Tome III, Travaux et Memoires, now gives—

$$\text{Type } I_0=1^{m}.00007604 \text{ at } 0° \text{ C}$$

a value $8^{\mu}.4$ greater than the preceding one, and accounting for the larger part of the change in the value of U. S. (Repsold).

Nothing could show more clearly the importance of the work the International Bureau is now doing than the fact that the value of one of their principal meters, supposed known in 1880 within 1^{μ} or 2^{μ} has since had its value increased by the $\frac{1}{120000}$ part.

This increase in the value of type I, resulting from recent direct comparisons of I_2 with the meter of the archives corresponds to a similar diminution in the length of the ideal meter. But the change from Colonel Clarke's value of the meter in terms of the yard, to the value found above, corresponds to a reduction in the length of the meter of about $\frac{1}{70000}$ part, and the recent change of $\frac{1}{120000}$ in the value of type I accounts for but little over one-half of this $\frac{1}{70000}$.

It seems probable that a considerable part of the discrepancy must be due to errors in the values heretofore used of the ratio at different temperatures of the meter of the archives to the toise of Peru and its derivatives.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

THIRTEENTH MEMOIR.

ON COMPOSITE PHOTOGRAPHY AS APPLIED TO CRANIOLOGY; AND ON MEASURING THE CUBIC CAPACITY OF SKULLS.

ON COMPOSITE PHOTOGRAPHY AS APPLIED TO CRANIOLOGY,

By J. S. BILLINGS;

AND ON MEASURING THE CUBIC CAPACITY OF SKULLS,

By WASHINGTON MATTHEWS.

READ APRIL 22, 1885.

At the last annual meeting of the National Academy, we presented, through the courtesy of Major Powell, a preliminary communication upon the application of the method of composite photography to the study of craniology.

Experiments in this direction have been continued at the Army Medical Museum during the past year, and we have arrived at what seems to be a fairly satisfactory method for centering successive skulls in a series, in order that the images of each may be properly superimposed in the camera. This might be done in a great variety of ways; but the one upon which we have settled, the details of which have been worked out by Dr. Matthews, is as follows:

The camera stand and "patent lever adjustment-gallery stand," for the object are leveled with a spirit level and the tops of both stands are adjusted at exactly the same height. Two fine black lines, one horizontal and one vertical, are drawn from margin to margin on the ground glass focusing plate, intersecting in the exact center. On the object stand are placed two frames on which intersecting threads are stretched, exactly parallel with the lines in the camera plate, so that they may be covered by the latter when focused. Besides these cross lines there is a vertical thread, stretched on a separate frame, lying in the same plane as the other vertical threads and the vertical line on the camera plate. The craniophore is placed on the object stand behind the first or anterior frame at such a distance that the facial bones of the longest skulls will not interfere with the cross threads. The second or middle frame—that which bears a vertical thread only—is 22 centimeters behind the first frame and is, of course, behind the craniophore. The third or posterior frame—a vertical board with a central opening 16 centimeters square—stands 50 centimeters behind the first frame; to its top attached by one margin, is a screen of black velvet, which is raised while the skull is adjusted and dropped while the exposure is made. The posterior frame is fixed in its vertical position; the other two frames are attached to the stand by hinges, and are lowered during the exposure. A graduated rule is placed by the side of the craniophore and is photographed with the skull so that scale of each picture may at any time be ascertained.

After making sure that the threads are properly adjusted to correspond with the lines on the ground glass plate, the sensitized plate is inserted, the cap put over the camera and the plate cover withdrawn. A skull is put on the craniophore and adjusted, the first and second frames are lowered, the velvet screen let down, the cap removed and a fractional or partial exposure is made, the time being regulated by the metronome. The cap is then placed over the lens, another skull is adjusted on the craniophore and another fractional exposure made, and so on until all the skulls of the selected series have been photographed on one plate which is not removed from the camera until the last exposure is complete. The focal distance is the same for each skull.

The plane and points by which the skulls of the later series have been adjusted are the German horizontal plane, the subnasal point, the supra-auricular point, and the maximum occipital point. For the front, the rear, and the side views we adjust the German horizontal plane to correspond with the plane of the horizontal threads, while the subnasal and maximum occipital points (or the supra-auricular points as the case may be) are brought into the plane of the vertical threads. In preparing for the front view we take sight on the horizontal plane and the subnasal point from the front—*i. e.* the side next the camera—and on the occipital point from behind through

the opening in the third frame. In preparing for the rear view we take sight on the horizontal plane and subnasal point from behind, and on the maximum occipital point from before. With the side view the facial portion of the skull is turned toward the left. We take sight on the horizontal plane from a position to the left of the center; on the left supra auricular point from before, and on the right supra-auricular point from behind. With the described apparatus views of the base and vertex have not yet been attempted, but it is believed that lateral frames with the usual cross-threads must be added to secure good views of these aspects of the skull.

The duration of each fractional exposure depends on many conditions; the sensitiveness of the plate, the refractive power of the lens, the orifice of the diaphragm, the degree of light, the color of the skull, and the number of skulls in each series. In a series of five skulls, other things being equal, each fractional exposure will be twice as long as in a series of ten. In photographs, 19 *et seq.*, we used "Carbutt's Keystone dry plates," a Dallmeyer triplet, 4½-inch lens, a diaphragm with 1¼ inch aperture, and an exposure of from 10 to 20 seconds for each skull.

Different methods of determining the adjustment of the skulls have been tried: First. Two sets of cross-threads have been used, and the operator taking sight only from the front, with his head placed immediately in front of the lens. Second. Two sets of cross lines employed, the operator looking through the camera only, the plate necessarily removed from the camera before each exposure. Third. Two sets of lines as before; an accessory camera used at the side; the front vertical thread aligned on the more distant margin of the anterior nasal orifice. Fourth. Two sets of lines used; a sketch of the first skull drawn on a cross-lined gelatine film, and each subsequent skull made to conform as nearly as possible to this sketch. Fifth. The plan already described at length and at present adopted by us, in which there are four sets of lines—one on the ground-glass plate—and which the operator views not only from the front, but from behind, through the opening in the posterior frame, in order to secure a proper alignment of the maximum occipital point in the front view, and of the subnasal point and the horizontal plane in the rear view.

The apparatus used, which is illustrated in the plates, is rudely improvised from material at hand. The frames were not made on purpose, but were such as we had in the museum for other uses. A more convenient apparatus is to be constructed, but the general principles of the one now in use will be preserved.

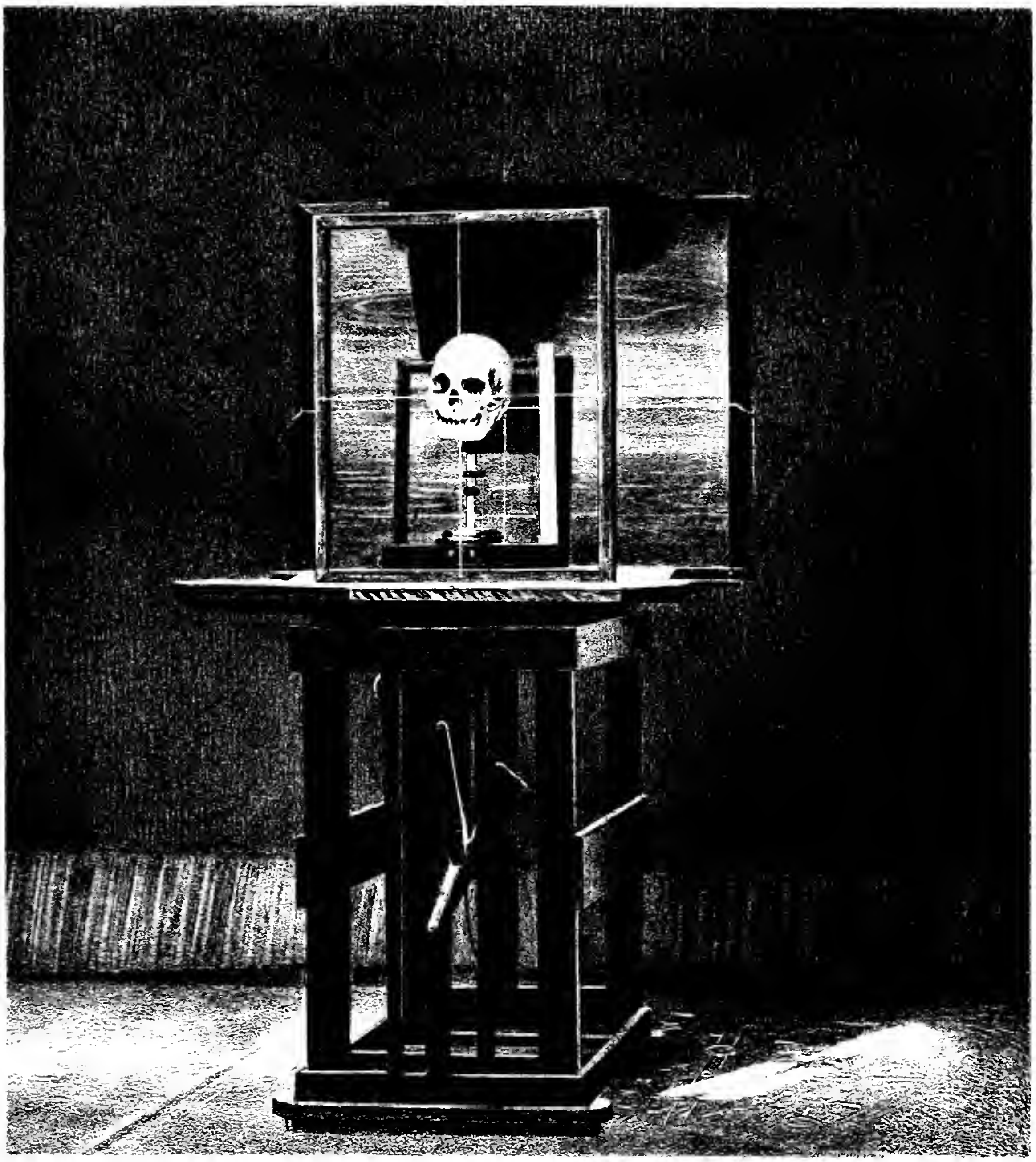
Our craniophore, however well it may be adapted for the purpose for which it was originally intended, is not well suited for adjusting skulls in photography. The modification of this, recommended by Ranke figured in "Archiv für Anthropologie, 1883," would undoubtedly do better, but a still more suitable craniophore can, we believe, be devised, and we propose to have such an instrument constructed.

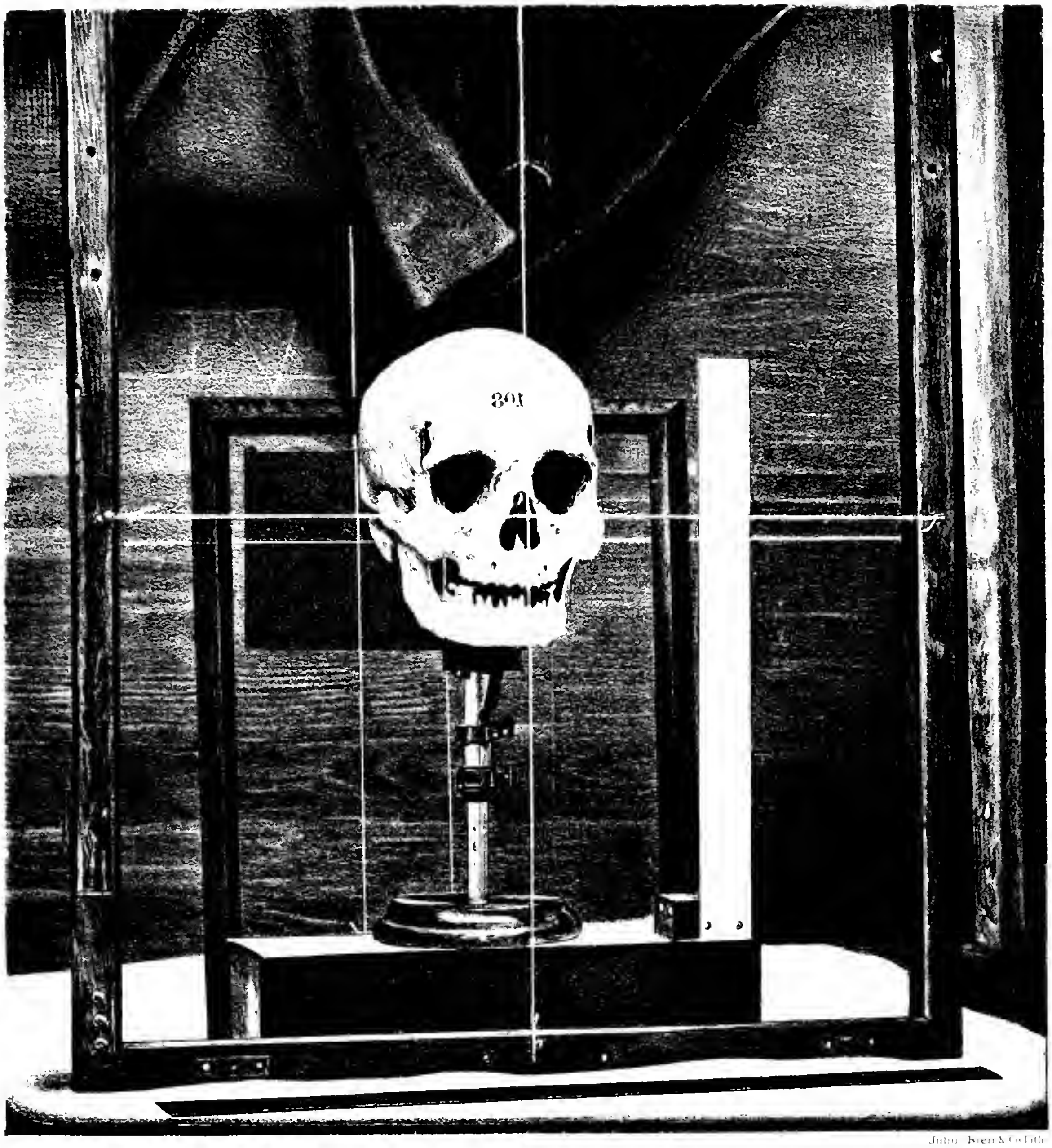
Two sets of composite photographs of crania are shown herewith, viz: One set including six male Sandwich Islanders' skulls, and one set including six male Arapahoe Indian skulls. There are six photographs in each set, and all are exactly half the size of the original objects.

The value of this method of composite photography, as applied to craniological studies, depends, to a very considerable extent, upon the adoption of some uniform standard of size for the preparation of such photographs, in order that the series of specimens in different museums and collections may be directly compared. It appears to me that the most convenient scale for such photographs is to make them of one-half their natural size—that is, so that the inch divisions on the graduated rule, which is always photographed with each set, shall measure exactly one-half inch in the photograph.

These composite photographs must be studied in connection with the measurements of the crania represented in them. The method is simply a rapid and convenient means of obtaining a graphic representation of a series of irregular objects, a picture which should indicate not only the mean size and shape of these objects, but also, to a certain extent, the maxima and minima of their variations. I think it is much better to make the photographs directly from the skulls themselves, than to construct them from separate photographic prints of each skull; which appears to be the method pursued by Dr. Thomson, of Edinburgh, in the specimens given by him in the "Journal of Anatomy and Physiology," London, 1885, Volume XIX, Pt. II, page 230.

It is well known to ethnologists that the distinctions of race are much more marked in the physiognomy of the living subject than in the differences shown by dried crania; and that the



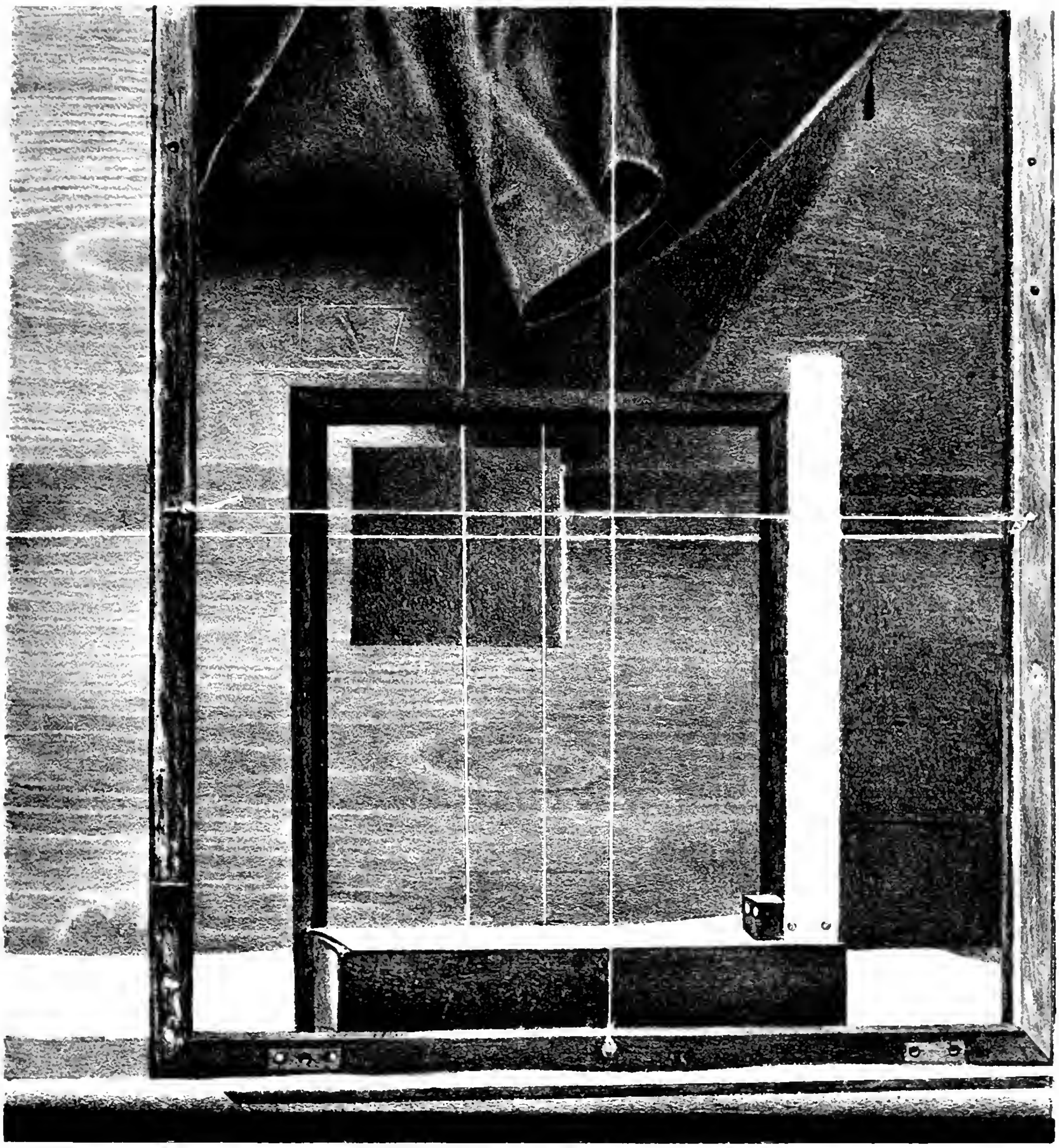


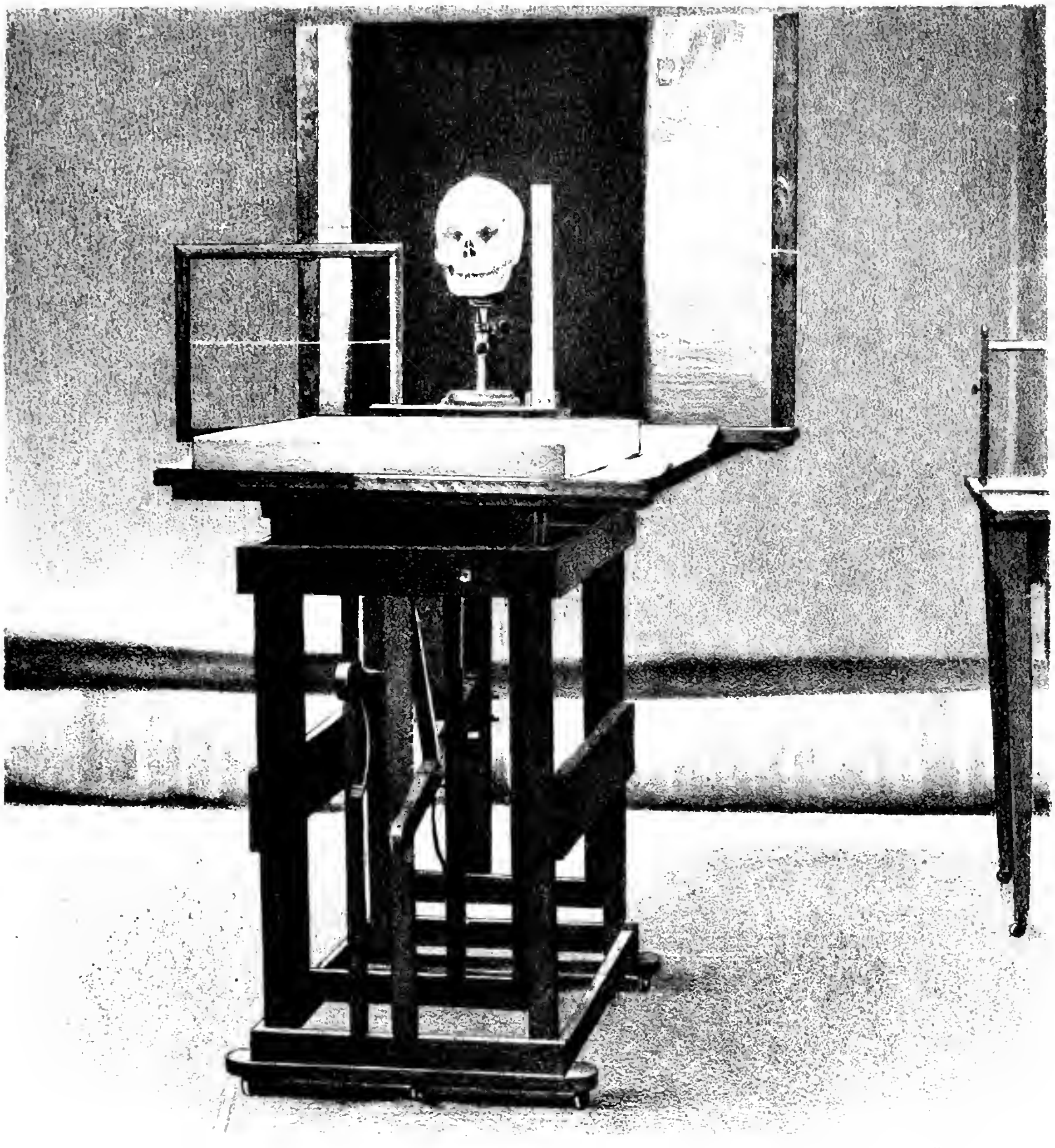
Julius Bien & Co. Lith.

ARRANGEMENT FOR TAKING

COMPOSITE PHOTOGRAPHS OF SKULLS

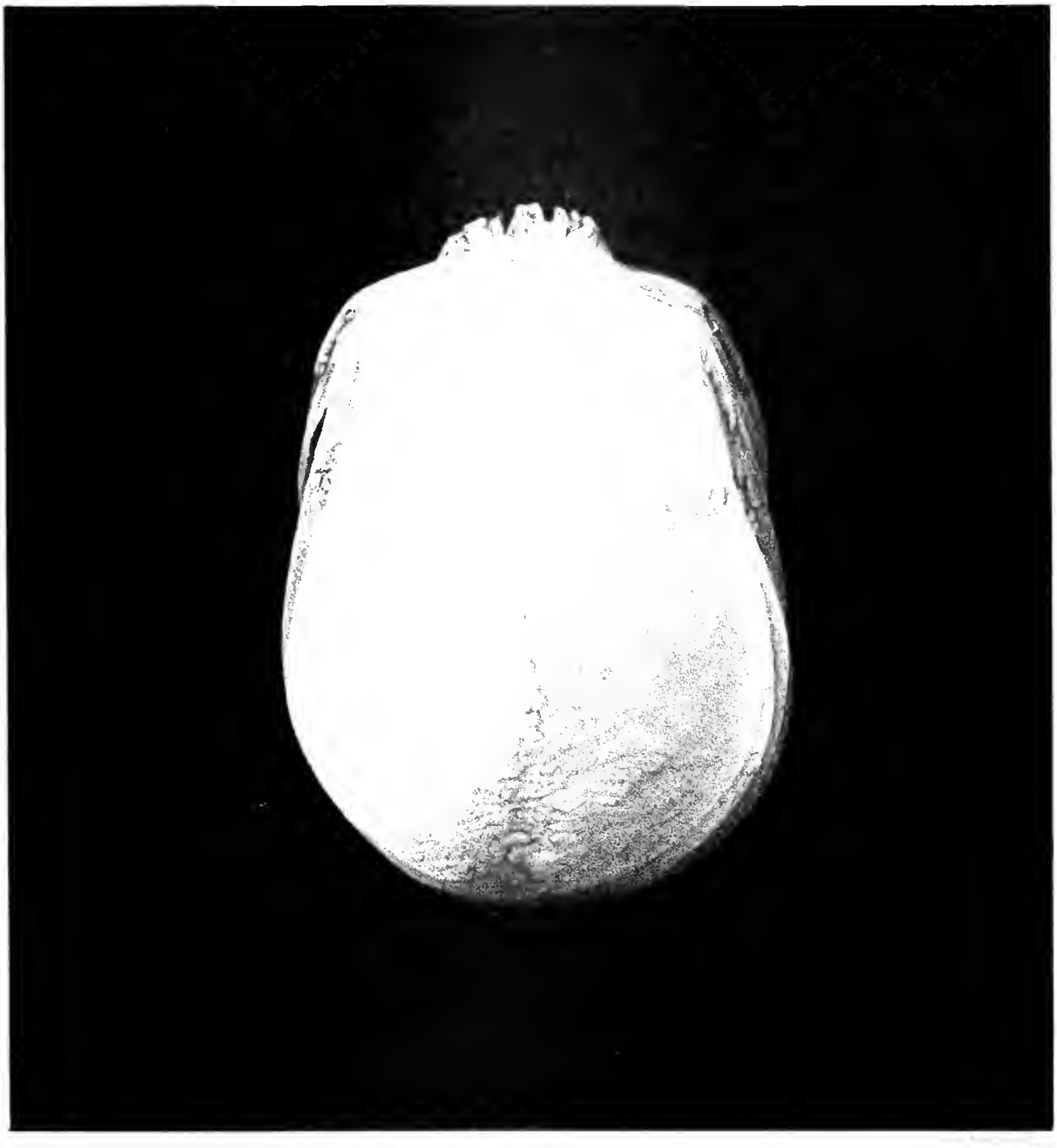
PHOTOGRAPH NO. 2. Same as No. 1 — nearer view, skull accurately adjusted for photographing.

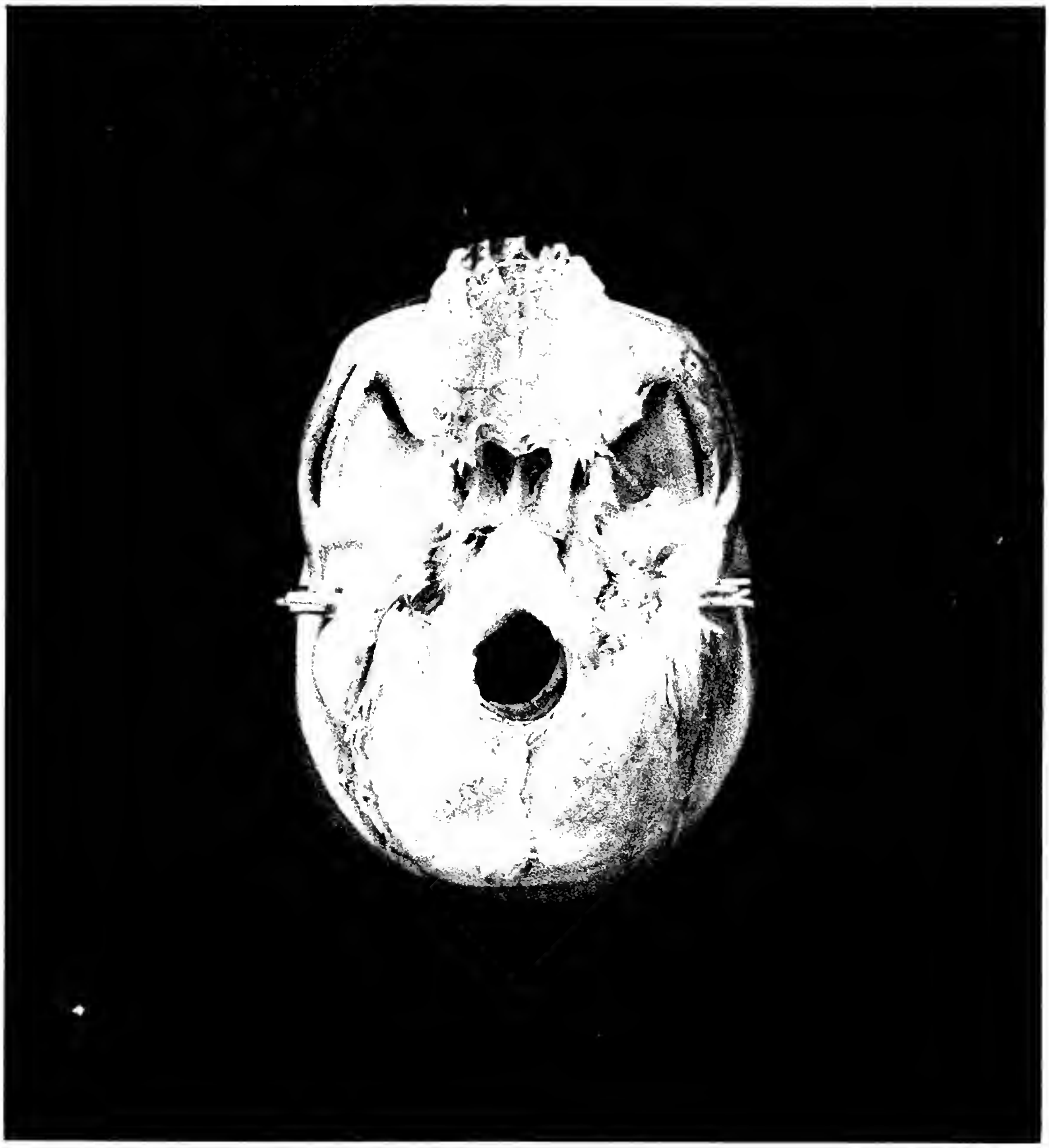


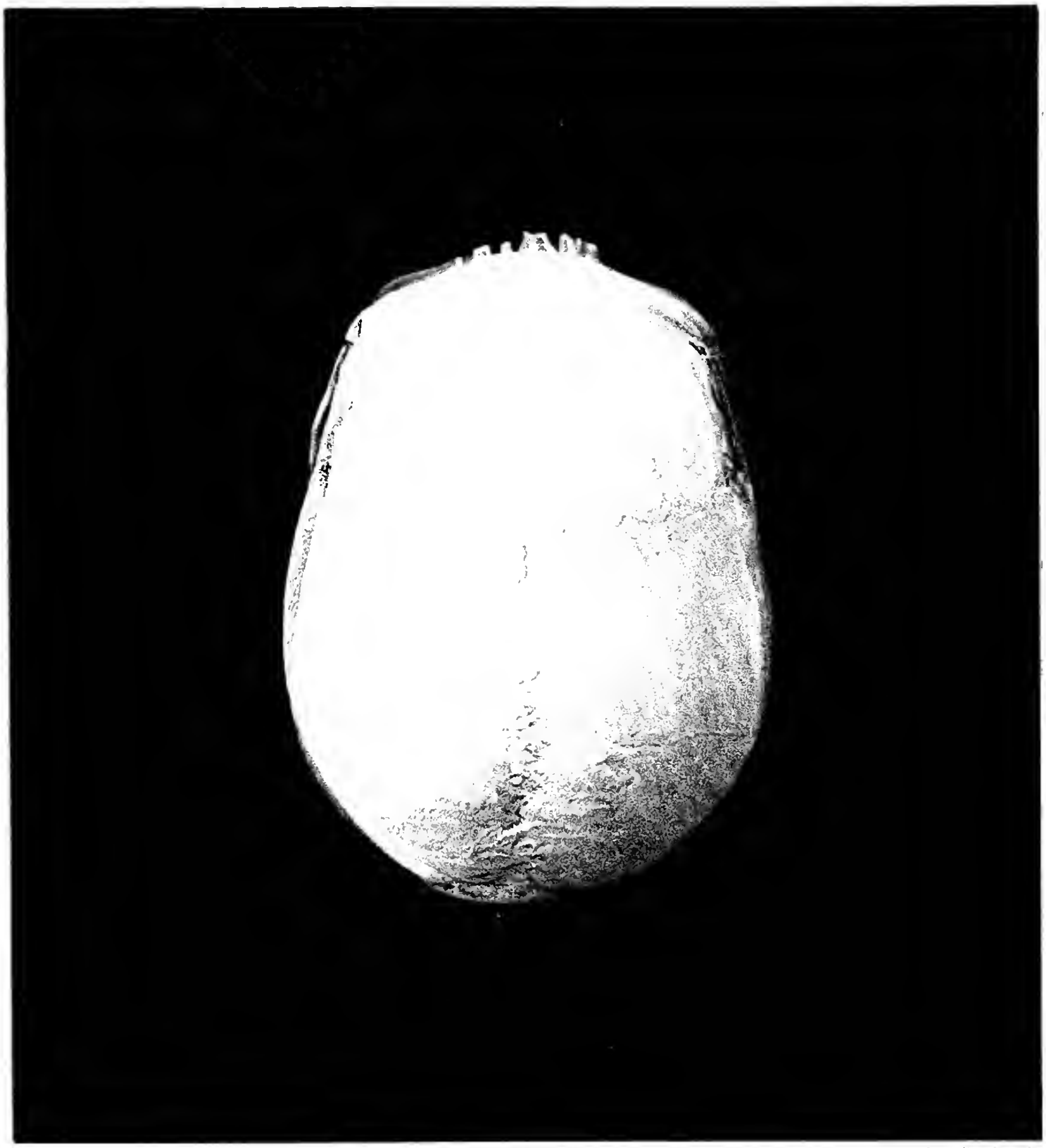


APPARATUS FOR TAKING
COMPOSITE PHOTOGRAPHS OF SKULLS

PHOTOGRAPH NO. 1.—Shows everything ready to make the exposure. The [illegible] and middle frames [illegible] and the [illegible] screen has been dropped.

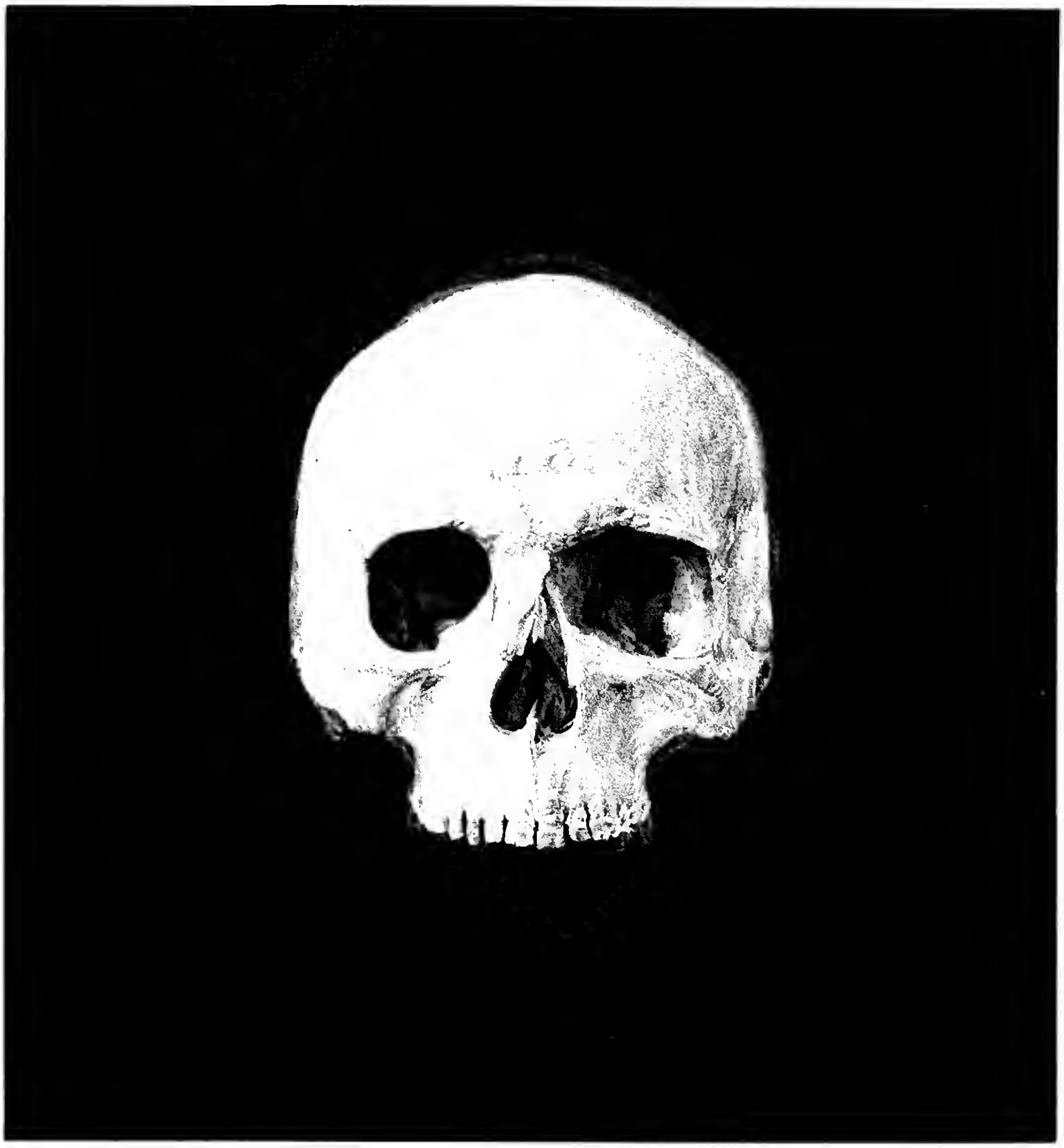


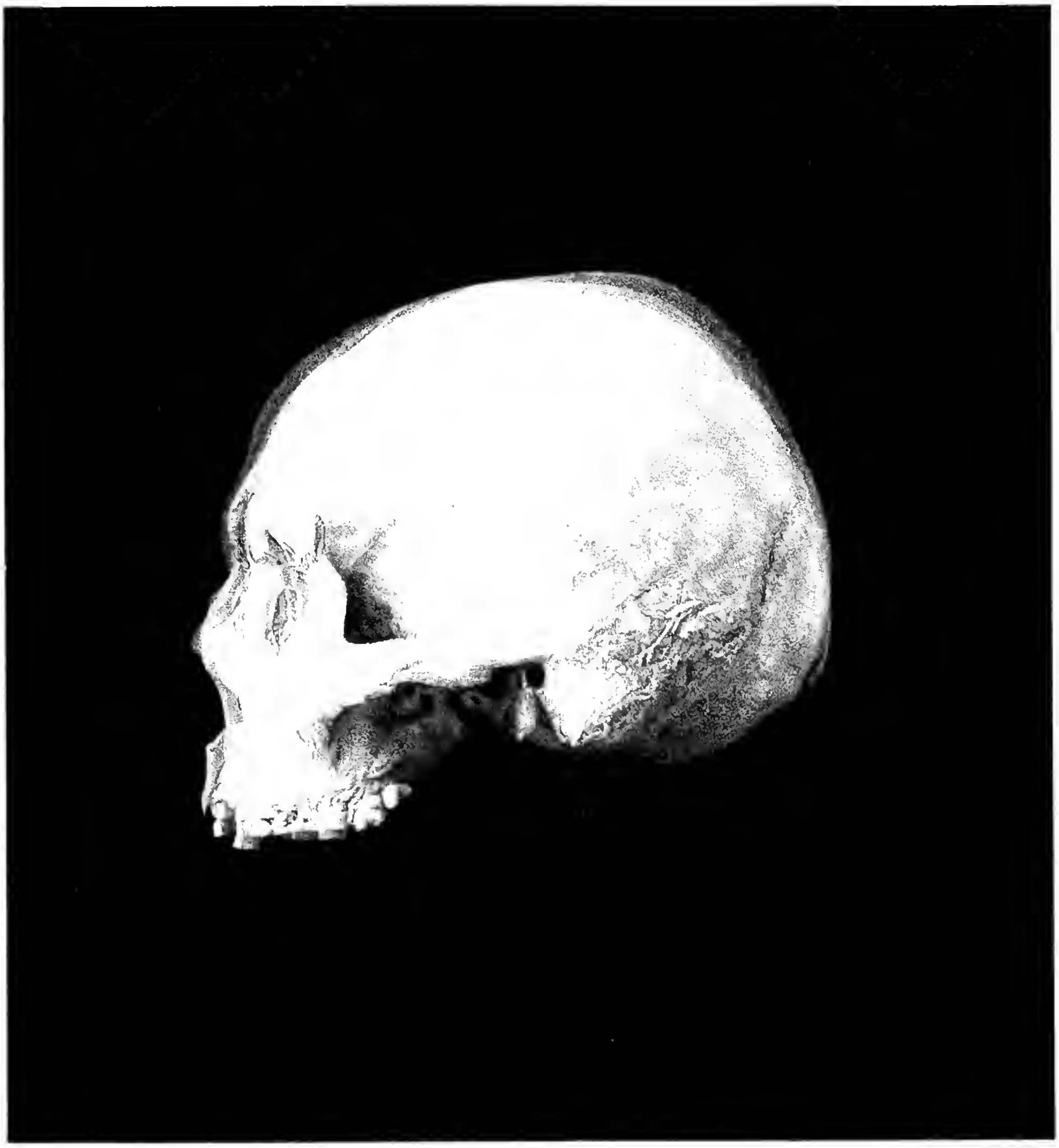


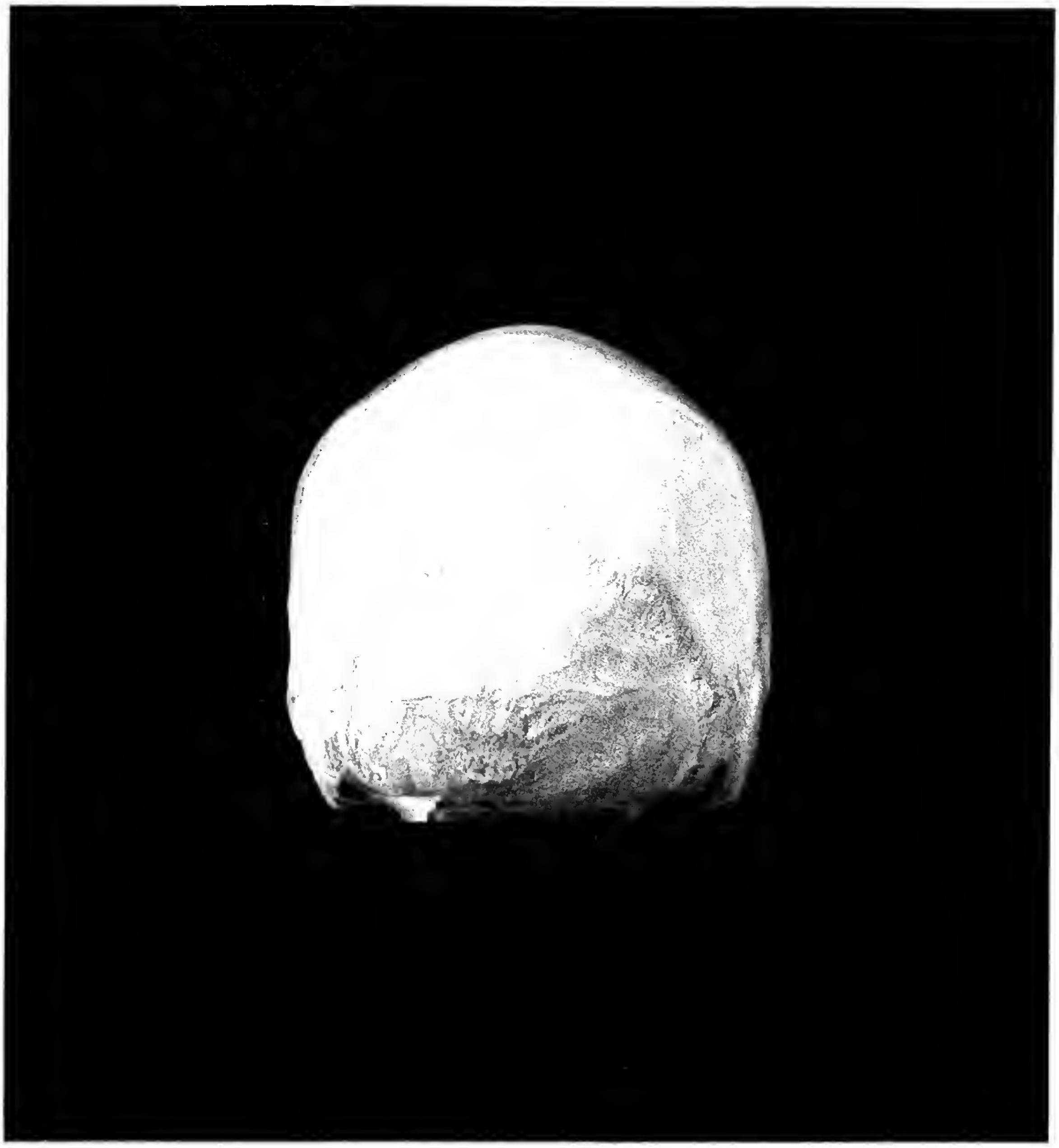


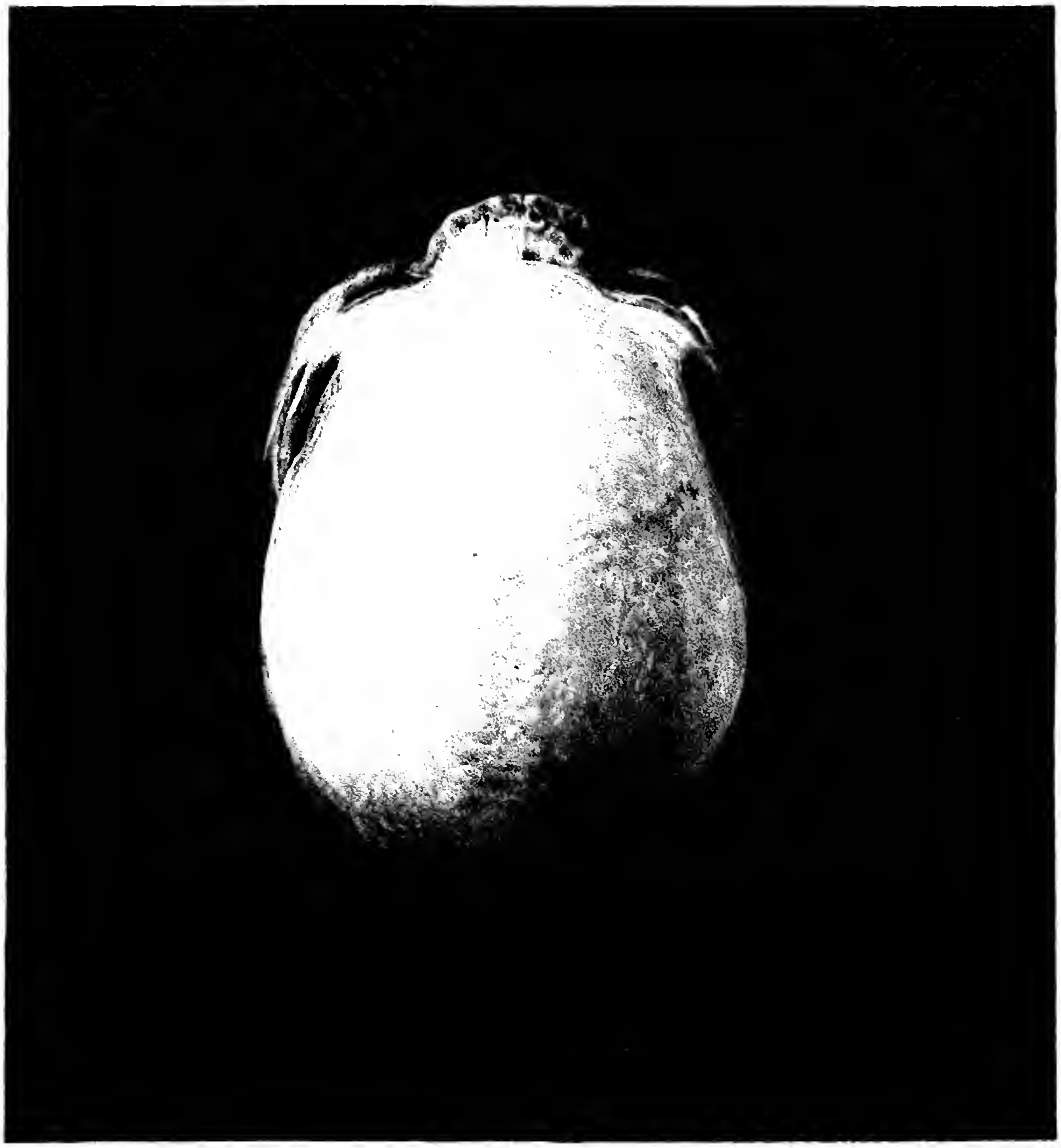
... ADULT MALE ... SKULL.

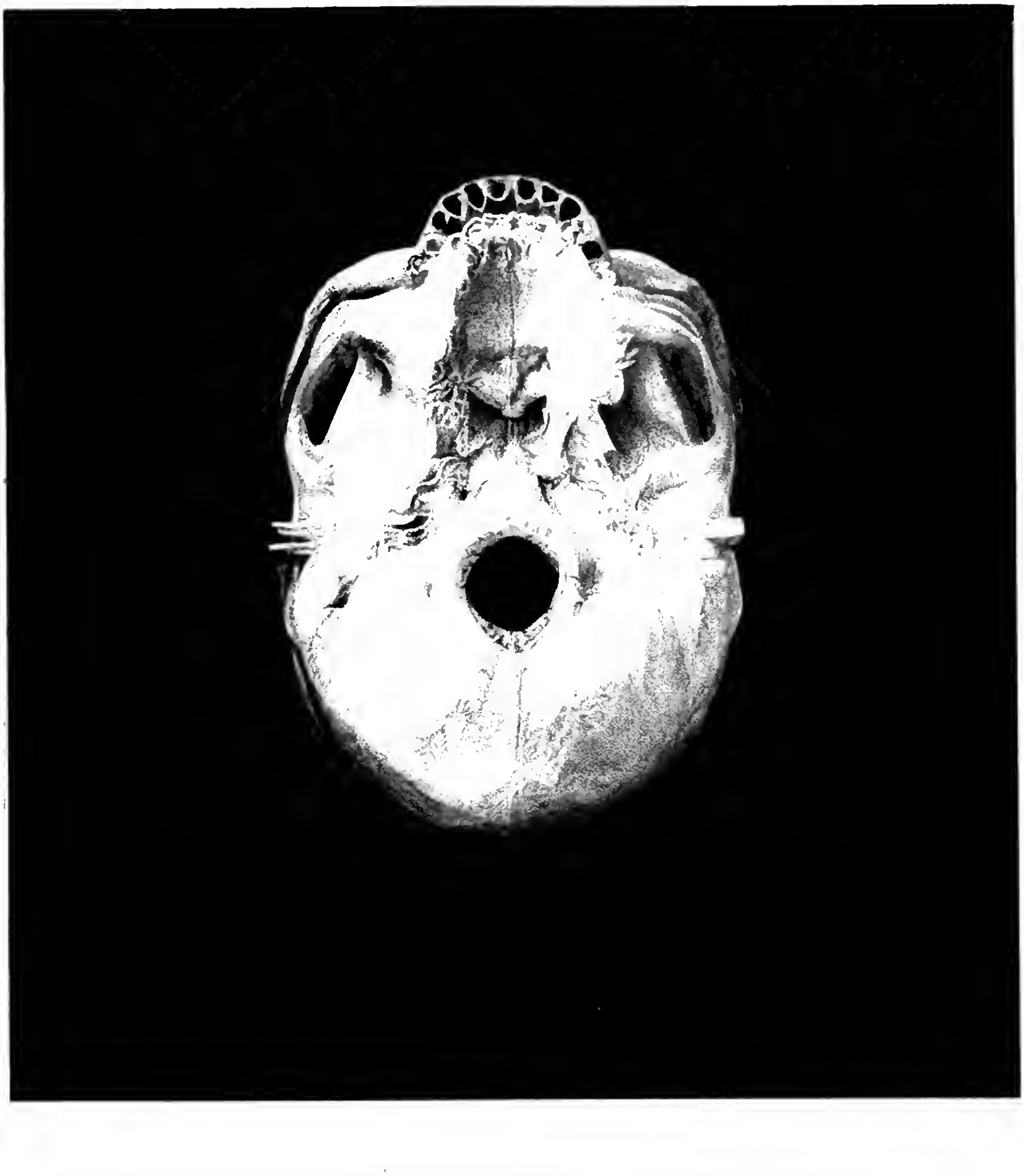
... Normal ... plane conditions same as No ...











bones of the face with the relations which they bear to those of the calvarium, give more valuable race indications than do the calvaria alone. While something has been done in the study of the internal configuration of the cranial cavity, and more especially of the various fossæ and projections at its base, with reference to their differences in various races, this field of inquiry is as yet comparatively unworked. It seems very desirable to follow out this special line of investigation in connection with the large and valuable collection of crania of American races which now exists in the Army Medical Museum and in the National Museum. To do this, however, it is necessary that sections should be made of the skulls, and before making such sections it is desirable that all measurements and especially the measurements of cubic capacity of these crania, should be made according to the best and most approved methods, and the results carefully recorded.

From the results of some preliminary experiments upon the methods most in use for measuring the cubic capacity of crania, I became much dissatisfied with their accuracy, and accordingly requested Dr. Washington Matthews, my assistant at the Museum, to undertake a series of experiments for the purpose of obtaining, if possible, some more accurate and reliable method of ascertaining the cubic capacity. I think that he has succeeded, to a very great extent, in devising a perfected method which accomplishes this result, and I have the honor to present to the Academy, by permission of the Surgeon-General, a full report, prepared by Dr. Matthews at my request, embodying the results of his observations and experiments.

SURGEON GENERAL'S OFFICE, ARMY MEDICAL MUSEUM,
Washington, D. C., April 14, 1835.

SIR: I have the honor to report, as directed, on the experiments which have recently been made in the Army Medical Museum to test the practicability of finding the cubic capacity of the cranial cavity by means of water. I will review briefly the general reasons which led us to perform these experiments.

The labors of anthropologists have been largely directed to perfecting methods in which solid particles are used, but the laws regulating the fall and subsidence of granular substances are imperfectly understood and every change of condition and manipulation produces a change in the space occupied by them. True, Broca has formulated certain laws which govern the flow, distribution, and subsidence of solid particles, or, as Dr. Topinard calls them, granular bodies. But these laws are of limited application, "all bodies do not obey them with equal regularity," and, notwithstanding the accurate rules he lays down to govern our procedures in handling these bodies, it seems almost impossible for any two persons, by merely reading his instructions to arrive at the same results; for Dr. Paul Topinard, the famous disciple of Broca, in his latest great work, says: "Published documents on the capacity of the skull can only be used with extreme caution. As a general rule only the results obtained by the same hand or by the immediate disciples of the same authority should be compared."* The laws of granulistic physics have been but recently studied, have been studied by few men, and are still imperfectly known. This is not the case with hydrostatics and hydraulics; there are no sciences more widely or well understood, none which have longer formed a subject for study to our race. If, therefore, water could be used as a medium for cubing skulls, the perfect knowledge we possess of the laws which govern its motions would be of of vast advantage to us.

This is no new idea. A careful search, made in all the papers on the subject that we could obtain, showed that experiments had been made in this direction but without satisfactory results. Skulls had been rendered waterproof but at such a great expense of time and labor that when the best results were obtained, the skulls were available only for standards, by means of which to study or regulate measurements by other methods. As the conclusions of those who had previously experimented with water are epitomized by Dr. Topinard in his work already referred to, I cannot do better than quote his words, which are as follows: "The most simple and most direct procedure is that of water. All the exterior orifices except the occipital foramen are closed with wax, the cavity is filled with water, and to do it well, with distilled water at 4 degrees if the absolute weight is to be determined, and at 14 degrees temperature, at which measuring glasses are

* Éléments d'Anthropologie Générale par le Dr. Paul Topinard. Paris, 1855, p. 609.

graduated, if the volume is to be determined; the water is emptied into a measuring-glass of 2,000 cubic centimeters and you read; there is the difficulty, the water wetting the sides of the glass rises on it, and one can be mistaken to the extent of five cubic centimeters. Another cause of error, which is more important, is in the water wetting the walls of the skull itself, soaking and penetrating through the internal free orifices as far as the vacuoles and sinuses. If this water remained in the walls it would only be a half evil; but when the skull is drained a part comes out of the sinuses and vacuoles and is unduly measured with what comes out of the cavity proper.

"Instead of directly seeking the volume we can proceed by weight. We weigh the skull full, then the skull empty; the difference is the weight of the water; but comprised in it is that which the vacuoles and sinuses contain. There is only one means of remedying this cause of error, it is to saw the skull, to cover it internally with an impermeable coat and to join the two halves. This can succeed; but in spite of the care exercised in the preparation one cannot guarantee that the water will not again infiltrate the walls unperceived. If instead of measuring the water as it comes out of the skull it is measured as it goes in, the same causes of error persist."*

Elsewhere he speaks of the use of those sawed and carefully varnished skulls as terms of comparison; but even in this capacity he condemns them.†

A careful consideration of all the literature attainable on the subject of water-measurements led us to conclude that the experimenters had been too easily discouraged, had not sufficiently contended with the difficulties which the problem presented, and that we still had a good field for investigation.

In seeking for a substance with which to coat the skull and render it water-tight, the merits of fresh putty, to be removed before it hardened, were suggested, and it was first applied on the 30th of last June. At first it gave by no means perfect results; yet it seemed to promise so much that we determined to persevere in its use. It appeared from the beginning that our chief difficulties were lack of dexterity in applying the putty and the hygroscopic nature of the osseous tissue. A number of experiments were performed, the causes of error noted and means devised to remedy them. It is needless to recount all our mistakes and the various stages in the growth of the system. I will, therefore, proceed at once to describe our present methods and appliances, and, with these fully explained, the merits and demerits of the system can be more easily understood.

The following are the necessary implements and materials:

1. Scales and weights.

2. An ether spray-apparatus of the pattern known as the reversible spray-apparatus with revolving spray-tube.

3. A bottle of shellac varnish, made by adding one part by measure of dry gum to nine parts of strong alcohol.

4. A roll of Seabury and Johnson's India rubber adhesive plaster.

5. A quantity of putty; at least ten pounds should be kept in store.

6, 7, 8. Simple cerate, lard and linseed oil.

9. A bread-board and rolling-pin with which to work the putty.

10. A covered jar containing water in which to preserve the putty when it is not in use.

11. A reservoir of water provided with India rubber tubing and stop-cock. The reservoir now in use in our laboratory has a capacity of about 16 liters, is elevated $1\frac{1}{4}$ meters, has a tube 2 meters long and of 13 millimeters caliber, and a stop-cock of 5 millimeters caliber; but these are not essential details.

12. An ordinary tin half-gallon measure, half-covered, and provided with a spout 3 centimeters in diameter.

13. A wide shallow pan provided with a lip, for receiving the water from the skull and transferring it to the measuring glass. The pan we use is 36 centimeters wide and 8 centimeters deep.

14. A metronome, set to count seconds.

15. A measuring glass graduated for 2,000 cubic centimeters, such as that adopted by Professor Ranke, of Munich.

* TOPINARD, *loc. cit.*, page 592.

† ID., *loc. cit.*, pages 597, 598.

16. A wiper, consisting of a sponge tied to a stout rod, to dry the measuring glass after each measurement.

17. An insufflator.

18. A quantity of lycopodium in a convenient box or bottle; or a mixture of lycopodium and charcoal.

19, 20, 21, 22. Implements for removing putty, from fossæ and foramina. We use dressing forceps, tenaculum, scoop, and nail-brush.

23. Thermometer.

Procedure.

1. For this and all other methods of cubature of cranial cavities it is well to wash them out carefully first. In making measurements with granular bodies the value of cleanliness may have been overlooked and its absence may have proved an important source of error. In measuring with water it has been observed that much dirt comes from some skulls. One complaint made against methods where shot is used, is that they are dirty. This complaint would cease if the skulls were washed.

2. Before being washed or measured the skull should be weighed and the weight recorded. After washing it should be left for some weeks to dry, until it again weighs exactly the same as before it was wet. This is to assure against increase in cubic capacity from absorption of moisture.

3. Spray the inside of the skull uniformly and completely with the shellac varnish by means of the reversible ether spray apparatus, taking care that the anterior and middle fossæ are not neglected, as they cannot be seen so well as other parts. Use exactly 10 cubic centimeters of the varnish; this amount has been found sufficient to give the skull a complete coating; if more than this amount is used at one time it is apt to pour out through the sutures. This quantity, too, will leave exactly one centimeter of gum in the skull to be considered when we come to the cubing. It will not, however, alter the results if another measure of 10 centimeters of varnish is used after the first coat dries, care being taken to add one centimeter to the measurement for the additional gum put in the skull. The pendant portion of the skull should be often changed while the spraying goes on, lest the varnish accumulate in one spot and flow out on the external table through some open suture. The largest skulls may be well varnished by this means in about three minutes. It will be found a great saving of time to spray a large number at one sitting, as all the apparatus used in this work must be thoroughly washed with alcohol before being laid aside. When the spraying is complete the skull should be allowed to remain, before measuring, long enough for the alcohol to evaporate and the varnish to harden; for this, in our experiments, at least twenty-four hours in a warm room has been given, but it is probable that a much shorter time would suffice.

4. The skull is examined, and if any artificial holes are found in its parietes, they are covered with pieces of suitable size of the India-rubber adhesive plaster. The sphenoidal fissure and the entire apex of the orbital cavity is also closed with a piece of this plaster, about an inch square, well forced into place.

5. Before the piece of plaster is put on the sphenoidal fissure, a piece of putty, sufficient to fill it and no more, is pressed into the optic foramen. The orbits are then entirely filled with putty. The carotid canal is next filled from its external opening, and the putty is pressed until it appears or is felt at the inner opening of the canal, or until no more will enter. The operator puts his index finger in at the foramen magnum, and places the tip, in turn, in contact with the internal orifice of each foramen of the base; he holds it there and presses a piece of putty into the foramen until he feels the putty coming in contact with his inserted finger; by this means he knows that the foramen is filled, and yet that the true cranial cavity is not encroached on. The condyloid foramina and the internal meatus are filled from within. He may put a piece of plaster over the meatus instead of the putty. Next the nares are filled completely with the putty, and it is important that the substance should be well forced up to the base of the cribriform plate of the ethmoid. Next the spheno-palatine and temporal fossæ are filled, the putty being well pressed into all parts. If the squamous suture or other sutures are open, a little may be pressed into them, care being taken that none is forced into the cerebral cavity proper. The base of the cranium from the foramen magnum to the alveolar process is next liberally covered with putty applied with

barely sufficient pressure, except on the palate, to make it stick; if too much pressure is applied it will force into the cranial cavity the material in the foramina at the base of the skull, which has previously been applied with such great care. Next roll out on the bread-board, with the rolling-pin, a sheet of the putty of a size sufficient to cover the vertex of the skull. It should be of uniform thickness—not less than an inch throughout. Now the skull is laid on the table, base downwards—and this position is important—the cap of putty is placed on the vertex, pressed closely on and worked around until it completely covers the cranium, leaving only the superior alveolæ, parts, perhaps, of the zygomatic arches, and the margin of the foramen magnum exposed to view. The completed, the skull is ready to be filled.

6. In the mean time, or before you begin operations, you have the half-gallon measure filled to within about 3 centimeters of the top. The skull is now held in the hands of an assistant, base upwards, in such a manner that the plane of the foramen magnum shall dip forward at an angle of 45 degrees or more with the horizon. This is necessary in order that, as the water rises in the skull, air may not be imprisoned in the middle cerebral fossæ. I have seen an error of 10 centimeters result from a neglect of this precaution. Now take in the left hand the tin vessel of water and have the stop-cock either in, or convenient to, the right hand. Empty the water from the vessel into the skull through the foramen magnum as rapidly as you can, without spilling, until the skull is nearly full. Then rapidly lay down the tin measure, open your stop-cock and complete the filling of the skull, taking good care that you fill it at once rapidly and exactly. Much depends on the care with which the last few drops are added. As the assistant sees the water rising to the edge of the foramen magnum, he will gradually elevate the anterior portion of the skull until the plane of the occipital foramen is horizontal, and when the stop-cock is opened he will bring the skull close to the edge of the pan so that the process of emptying may begin the instant the filling is done.

7. The moment you consider the cranium properly filled, close the stop cock and notify your assistant, who should instantly begin to pour out the water; this is best done by holding the skull in such a manner, occiput depressed, that during a greater part of the time the air may enter freely as the water runs out. I might convey an idea of the approved method by saying that the occipital region is held fixed and the superior alveolar region made gradually to describe an arc of 180 degrees, until at the end of the operation the base of the skull is downward. Once more the anterior portion is elevated so as to allow any accumulation in the anterior and middle fossæ at the base to come back to the foramen magnum, and again depressed and rocked a little to each side to empty the posterior fossæ. This completes the task of emptying. Not a drop of the subsequent drainage from the skull, no matter how abundant it may be, should be taken into account. The filling and emptying of the skull should be done as rapidly as is consistent with proper care. Upon this celerity depends as much as on anything else the correctness of the results. A person who has gained a little experience can fill a skull of 1,400 cubic centimeters accurately in 45 seconds and empty it in 15 seconds; both operations together should not occupy more than one minute. We have filled in 30 and emptied in 12 seconds, but for emptying we would recommend that just 15 seconds be always consumed.

8. Next comes the cubature: First wipe out the measuring-glass carefully, in case it is moist from a previous measuring, and then empty the water from the pan carefully into the glass; every drop that can drain out being allowed to fall. The measure is then placed on a carefully leveled table. A small quantity of lycopodium is put in the insufflator and blown on the surface of the water. This makes the true general surface of the water easily discernible and prevents us from mistaking for it the edge of the water which has been raised by capillary attraction on the surface of the glass. Then read off the number indicated and add one centimeter for the dry shellac in the skull.

9. Now take all the putty carefully from the skull, have the latter well cleaned and put it away in a dry, warm apartment for a week or more until it is as dry as it was before the measurement was begun; this is determined by again weighing it, then you measure it once more to verify your former experiment.

Some further comments on the appliances and proceedings must now be given, which could not be introduced before without sacrifice of clearness.

The varnish has been only recently employed. The propriety of using it was early thought

of, but no good means of applying it suggested itself until the reversible ether spray apparatus came under our notice; since then we have had time to experiment on only 10 skulls. Table II shows the result of these experiments. As far as they go they appear to mark a decided improvement in the process. But the improvement is probably not so great as the figures seem to indicate, for the measurements on the varnished skulls are our latest, and we have observed that our dexterity in pursuing our own method is daily increased by practice. The varnish does not entirely prevent the absorption of water by the skull, but it probably so retards the absorption and return of the water as to nearly eliminate the errors arising from these causes. Excellent results have been obtained without the use of the varnish—a maximum variation of 10 cubic centimeters, a mean of 5.20 cubic centimeters. (See Table I.) The application of varnish is a means that requires more experiment; perhaps the use of a different gum would be better, and perhaps it would be of advantage to use a larger amount, but in the majority of skulls this could not be done at one sitting. Furthermore it would be necessary to allow for a larger amount of solid gum in the skull when we come to measure.

The putty should be of firm consistency and as dry as may be compatible with due plasticity. If, in some dry skulls, it does not adhere well, the external table may be oiled a little. One slight trouble with the use of putty is this: In pressing it into and extracting it from the nasal fossæ it is impossible to keep from injuring the turbinated bones where they are present. In the majority of the skulls of our collection the turbinated bones are already so injured that it is unnecessary to exercise any care of them; but the advisability of preserving them in some cases has not been lost sight of, and a means has been devised to keep them intact when desired. This is to fill the nares with a semi-solid oleaginous substance that can be forced well up into the nasal fossæ without breaking the bones. Simple cerate will do well in warm weather and lard in cold weather. This filling should be used only in the nasal fossæ proper, and the coating of putty should completely conceal and sustain it on the outside. It is removed by passing a stream of hot water through the nose; and, if motives of economy prevail, the unguent can be skimmed off the water, preferably after the latter has cooled. A large skull will require all of the ten pounds of putty to cover it, smaller skulls proportionally less. To put on the putty properly and expeditiously requires some practice, particularly in filling the foramina at the base. It would be well for the beginner to apply it first for a few times to the base of the sawed skull, looking only at the outside until he thinks his task is completed; then let him inspect the inside of the fragment and see what sort of work he has made of it. Again he may proceed, looking from time to time at the inside to see how he is doing each part of the work. It is stated in the instructions that the skull must be placed base downwards when the cap of putty is put on the vertex; this may seem unnecessary, but experiment has shown it to be essential. If you place the skull vertex downwards on the sheet of putty and attempt to draw the latter up around the skull you will not make it stick closely one time in ten. The close adherence of the putty to the skull is of course of prime importance. If, when the skull is full, you observe a single drop leaking anywhere through or around the putty, your work is a failure, stop it at once, clean off the skull and put it away to dry for another day.

The bread-board and rolling-pin are those ordinarily used by pastry cooks. They are best if made of hard wood. They should be thoroughly oiled in the beginning and the oiling should be renewed from time to time. They should be well scraped with a blunt wooden instrument when the day's work is done. We have found that these implements in wood answer well enough, but perhaps it would be better to have a roller and slab of glass, china, or stone.

It is, as before intimated, important that the skull should be filled very rapidly as well as very accurately. Our present arrangements of tin vessel, reservoir, tube, and stop-cock are designed to attain these ends, but better means may perhaps be easily devised. To fill the skull entirely through our small stop-cock takes too much time, hence the use of a tin vessel with a wide orifice to put in the greater part of the water; but when you come to the last few drops at the brim of the foramen magnum, it has been found that they can be added more accurately and conveniently through the orifice of the small stop-cock fed from a reservoir not too high, for a strong pressure makes the water unmanageable.

The Ranke measuring-glass, as we received it, was not smooth and level on the bottom, and

this had to be remedied, as must be done in all cases. Of course it is essential that the axis of the glass cylinder should be perfectly perpendicular; so not only should the bottom of the stand be made quite even, but the stand on which the glass is placed to be read should be carefully leveled.

Different substances have been tried to define the surface of the water. Any dry impalpable powder of low specific gravity may do; many such powders would, no doubt, answer as well as lycopodium. Lampblack gives a beautiful and accurate line of demarkation, but it does not work well in the insufflator and is a dirty thing to handle. Charcoal tends to sink. A mixture of two parts of lycopodium and one of charcoal floats well and gives a more distinct line than lycopodium alone.

As it is stated that the measuring-glass of Ranke is graduated with water at 14 degrees centigrade, we have adopted this as the temperature of the water we use. But it is important that not only should the water, when taken from the reservoir, be of this temperature, but that the skull and all the vessels used should be of the same temperature. Indeed, it is well if the general temperature of the apartment in which the measurements are taken does not vary much from this standard. If water is taken from a vessel at 14 degrees, poured into a colder skull, thence through a colder atmosphere into a colder pan, and thence into a colder glass, it will not be at the standard temperature when you come to read, and will register too low. The reverse will be the case if the temperature of the vessels and surrounding atmosphere are higher than 14 degrees. If the temperature of the room has been maintained for some hours at 14 degrees, there is little doubt that the skull and all the vessels will be at the right temperature; but if the heat has been increased or diminished but a short time before you begin work, water of the proper temperature must be put in the vessel before the measuring begins; but of course the skull cannot be thus regulated.

It will be seen in Tables I and II that the second measurement was taken in many cases within a week of the first, as it was found that in this time the skulls were reduced by evaporation to their former weight. It seems they had ample time to contract, for we record only one case (21, Table I) where the second measurement was greater than the first. Nevertheless, since Broca maintains* that the contraction of a drying skull is not always in direct ratio to the loss of weight, it would perhaps be well to allow a longer time for drying than we have done, in an apartment maintained at a low temperature. Care must be taken, however, that the weight, and therefore the capacity, are not reduced below the original standard.

The entire work of applying the putty and measuring the skull need not occupy more than 15 minutes. The time necessary for gauging and cubature of the skull after the putty is applied need not exceed 3 minutes. The task of cleaning the skull may be deferred for some hours and left to an unskilled assistant. Two persons have with us been employed in doing the work of filling, one to pour in the water while the other held the skull, but I think means might be devised by which, if necessary, one person could do the work.

We will now consider the merits, difficulties, and disadvantages of this method and see how it compares with others.

Dr. Topinard says, in the passage quoted above, that one of the prime difficulties is that the water gets into the sinuses and vacuoles of the skull and returns when the skull is drained (égoutté); this is true, and if the skulls were drained in our system we would never arrive at comparable or uniform results. As for the larger foramina, we fill them with putty. The sutures, the sinuses, and the osseous tissue take up much water, some of which they part with in a few seconds or minutes, some of which remains for hours and days, and is finally only carried away by evaporation. Now, that which soaks into the bony substance remains there until lost by evaporation; that which reaches the closed sinuses, which I believe to be very little during the few seconds the skull is filling, does not get time to return in emptying, and that which enters the sutures is held there some time by capillary attraction and departs slowly. Again, it is the sutures and sinuses at the base of the skulls which are the most extensive and the most bibulous, and these, at the close of the operation for emptying, are held in such a position that they cannot part with their water before the cranial cavity is emptied. Observations taken on sawed skulls and on skulls having the external tables of the frontal and sphenoidal sinuses broken, seem to corroborate these

* Études sur les propriétés hygrométriques des crânes, considérées dans leurs rapports avec la craniométrie, Revue d'Anthropologie, Paris, 1874, iii, pp. 385 to 444.

statements. During the few seconds taken to empty the skull it is hardly possible that some water does not return from the sagittal, coronal, and squamous sutures to the cerebral cavity, but I am satisfied that it is an almost inappreciable amount.

Does all the water which properly belongs to the cranial cavity drain out in fifteen seconds? I am certain it does not. If you fill Professor Ranke's bronze skull with water exactly according to our directions, and empty it in fifteen seconds exactly according to our directions, you will find that about 6 cubic centimeters are still retained. It cannot be maintained that the natural skull, even when well varnished, will do better than this. If it ever becomes desirable in craniometry that the true and exact cubic capacity of the skull must be ascertained, this quantity, or some other quantity to be determined by experiment and computation, may be added to the amount in the glass; but this small amount may perhaps be disregarded so long as science demands only "comparable values, collected under the same conditions and effecting among them affinities conformable to reality."*

Another objection brought forward by Dr. Topinard, and already quoted, is that "the water wetting the sides of the glass rises on it, and one can be mistaken to the extent of 5 cubic centimeters." This would truly be a grave source of error if it were not so easily removed. Many plans for remedying this difficulty have been thought of, and the one we have recommended of scattering some light powder on the surface of the water will, we hope, give satisfaction to all who may try our method.

One of the most important obstacles in our way to success in the early stages of our investigations—an obstacle not brought forward in this connection by Dr. Topinard—was this: The water soaking into the walls of the skulls almost invariably causes them to expand and rapidly increases their cubic capacity. Table III will illustrate this. But it will be seen that in twenty-one skulls re-measured within five minutes, fourteen did not expand appreciably in that short time; two increased 5 cubic centimeters, three 10 cubic centimeters, and two 15 cubic centimeters, which was the maximum. These were unvarnished. Among nine varnished skulls tried, the expansion never exceeded 5 cubic centimeters in five minutes, and reached this only in three cases. Therefore, I think we are justified in concluding that in forty-five seconds, the time allowed by our method for filling a skull, the expansion from moisture is inconsiderable; and it is specified in our instructions that before the next comparable measurement is made the skull shall be reduced by drying to its former weight, and presumably to its former capacity.

The amount of water which a skull will hold in its meshes is much greater than one would suppose who had not made special investigations into the subject. Some idea may be gained of the hygroscopic capacity of the skull by consulting Table IV; but this subject has received such extensive treatment at the hands of Mr. Broca, in his paper already referred to, that it need not be further considered here.

One advantage of our method I conceive to be the elimination of much of the personal equation, which is such a disturbing factor in all other methods. There is little, if anything, left for muscular exertion to alter. With our most important operations the unchangeable element of time takes the place of the fickle element of vital force. In the most popular of all systems, that of Broca, the muscular action is chiefly limited to one part of the operation, that of ramming or thrusting. But hear what Dr. Topinard has to say of the personal effort in this case: "The same person * * * does not thrust the same, morning and evening, before and after meals, in thinking of his business or in giving his entire attention to what he does, in conversing, or in smoking for stronger reasons, two persons at a distance from one another, who have not given an example to one another, of different ages, one of sixty, the other perhaps young, one convinced of the attention he must pay to the operation, the other having read the description and imagining that it is very simple."

For uniformity of results I think the figures shown in the accompanying tables (I and II, particularly the latter) have never been excelled by any method, and in considering these it must be remembered, in favor of the system under which they were obtained, that it is new and seems still capable of much improvement.

* TOPINARD, *op. cit.*, p., 591.

We cannot claim rapidity as one of the advantages of the method we describe; it occupies more time, we fear, than any of the ways in which solid particles are employed; but time and trouble on the one hand should not be weighed too heavily against exactness and uniformity on the other, if it can be shown that this method possesses both of these advantages. If it eliminates the muscular effort, it does not eliminate the personal equation in other respects, for it requires for its proper performance a scrupulous care and a perfect patience.

One of the most desirable objects to be attained by any system of measurement of the cranial cavity is a comparability of the figures given by different persons in different parts of the world, who have not had the advantage of studying under one master or taking personal instructions in one particular laboratory. When this end is reached our data for generalization will be vastly increased. The advocates of the various methods where solid particles are used do not claim that they secure such universal comparability. Dr. Topinard says: "One must have seen the method practiced that he wishes to follow. One cannot indicate in writing the force that must be put in a tap of the hand or in a blow of the rammer. The forty-eight knocks that Mr. Ranke performs on the platform, with his great measuring glass full, are difficult to repeat with the same effect.* * * * It is not, however, certain that it [the method of Broca] is everywhere well understood and rigorously practiced, and I, for my part, would not dare to unite in one list the figures obtained here and there in its name. Amongst the authors in which I have full confidence in this connection I will quote M. Mantigazza, M. Schmidt, M. Ranke, M. de Torok, M. Merejkowski, and *all persons in general who have passed through the Broca laboratory*."† May we claim for the method we describe any higher degree of comprehensibility? May we hope that craniologists throughout the world can, by the mere perusal of our description, follow our method exactly in all its details and arrive at the same results? We can only offer conjectures, into which the elements of hope and egotism must enter too largely to render them of any value. We cannot answer these questions until some other students are found who will take the pains to give our method a trial.

Very respectfully, your obedient servant,

W. MATTHEWS,
Assistant Surgeon, U. S. A.

Surgeon JOHN S. BILLINGS, U. S. A.,
Curator Army Medical Museum.

* *Op. cit.*, p. 599.
† *Op. cit.*, p. 69. The italics are our own.

TABLE I.—*Showing measurements, in cubic centimeters, of twenty-five skulls not varnished.*

	Museum number of skull.	First measurement.	Date of first measurement.	Second measurement.	Date of second measurement.	Maximum difference.	Condition of skull.
1	83	1390	January 28	1385	February 4	5	Porous; weatherworn.
2	84	1425	January 27	1420	February 4	5	Very light and porous.
3	87	1300	January 29	1300	February 4	0	Sutures open; hole measuring 2 by ¾ inches; porous.
4	93	1300	January 29	1300	February 5	0	Porous and weatherworn; hole ½ inch in diameter.
5	199	1400	February 13	1390	March 14	10	Very light; roofs of orbits broken.
6	200	1300	January 26	1295	February 13	5	Several holes, measuring about 5 square inches in all.
7	292	1485	February 3	1475	February 11	10	Solid and heavy.
8	359	1450	January 30	1445	February 10	5	Porous and weatherworn; several holes.
9	362	1275	January 27	1270	February 5	5	Porous; weatherworn; sutures open, and several small holes.
10	363	1360	January 30	1355	February 10	5	Sutures much open; light and porous; weatherworn; several holes.
11	364	1335	January 30	1330	February 10	5	Very porous; holes; weatherworn; light; sutures open.
12	369	1230	February 3	1230	February 10	0	Very porous; eroded; fissured; several holes aggregating about 5 square inches.
13	372	1135	February 2	1125	February 9	10	Very porous and weatherworn; several perforations.
14	373	1455	February 2	1455	February 9	0	Porous; sutures open; many perforations.
15	374	1295	February 3	1290	February 9	5	Very porous.
16	375	1305	February 2	1305	February 11	0	Very porous; sutures open; four small perforations.
17	394	1275	February 2	1270	February 11	5	
18	481	1455	January 22	1445	February 5	10	Squamous; sutures open.
19	482	1410	January 22	1405	February 12	5	Light.
20	1516	1160	January 19	1155	February 6	5	Squamous; sutures open.
21	1517	1550	January 19	1560	February 6	10	Good and solid.
22	1804	1575	February 3	1570	February 1	5	Fine and solid.
23	1914	1285	February 9	1280	March 14	5	Good, but light.
24	1915	1450	January 26	1440	February 12	10	Roof of orbits broken; very light skull.
25	2034	1200	January 22	1195	February 12	5	Sutures open.

TABLE II.—*Showing measurements in cubic centimeters of ten varnished skulls, compared with measurements of the same unvarnished.*

	Museum number of skull.	Unvarnished. (See Table I.)			Varnished.			
		First measurement.	Second measurement.	Difference.	First measurement.	Second measurement.	Difference.	Dates of measurement.
1	199	1400	1390	10	1400	1400	0	March 26, April 2
2	359	1450	1445	5	1450	1450	0	March 23, April 3
3	362	1275	1270	5	1270	1265	5	March 26, April 2
4	373	1455	1455	0	1450	1450	0	March 24, April 2
5	375	1305	1305	0	1300	1300	0	March 24, April 3
6	481	1455	1455	0	1445	1445	0	March 24, April 2
7	1516	1160	1155	5	1160	1160	0	March 23, April 3
8	1914	1285	1280	5	1285	1285	0	March 27, April 3
9	1915	1450	1440	10	1440	1435	5	March 21, April 2
10	2034	1200	1195	5	1190	1190	0	March 26, April 2

Sum of differences, 45.10.
Average variation in unvarnished skulls, 4.5.
Average variation in varnished skulls, 1.

TABLE III.—*Showing increase of cubic capacity, in cubic centimeters, from absorption of water.*

	Museum number of skull.	First measurement.	Second measurement.	Time since first measurement.	Third measurement.	Time since first measurement.	Fourth measurement.	Time since first measurement.	Maximum variation.	Variation within five minutes.
1	83	1390	1390	5 minutes ...	......	....	......	....	0	0
2	84	1425	1440	5 do ...	1445	10 minutes ..	1475	24 hours..	50	15
3	87	1300	1300	5 do ...	......	....	......	....	0	0
4	93	1300	1310	5 do ...	1330	10 minutes ..	......	....	30	10
5	199	1400	(*)	....	......	....	......	....	......	.....
6	200	1300	1300	5 miuntes ...	1315	24 hours.....	1315	24 hours...	15	0
7	292	1485	1485	5 do ...	......	....	......	....	0	0
8	359	1450	1450	5 do ...	......	....	......	....	0	0
9	362	1275	1275	5 do ...	1305	24 hours.....	......	....	30	0
10	363	1360	1360	5 do ...	......	....	......	....	0	0
11	364	1335	1345	5 do ...	......	....	......	....	10	10
12	369	1230	1245	5 do ...	1250	10 minutes ..	......	....	20	15
13	372	1135	1140	5 do ...	......	....	......	....	5	5
14	373	1455	1455	5 do ...	......	....	......	....	0	0
15	374	1295	1295	5 do ...	......	....	......	....	0	0
16	375	1305	1310	5 do ...	......	....	......	....	5	5
17	394	1275	1275	5 do ...	......	....	......	....	0	0
18	481	1455	1455	5 do ...	1470	48 hours.....	......	....	15	0
19	482	1410	1410	5 do ...	1415	10 minutes ..	1425	48 hours...	15	0
20	1516	1160	1175	24 hours ...	......	....	......	....	15	......
21	1517	1550	1610	24 do ...	1610	48 hours.....	......	....	60	......
22	1804	1575	1585	5 minutes ...	......	....	......	....	10	10
23	1914	1285	(*)	....	......	....	......	....	......	......
24	1915	1450	1450	5 minutes ...	1470	24 hours.....	......	....	20	0
25	2034	1200	1200	5 do ...	1215	48 do	......	....	15	0

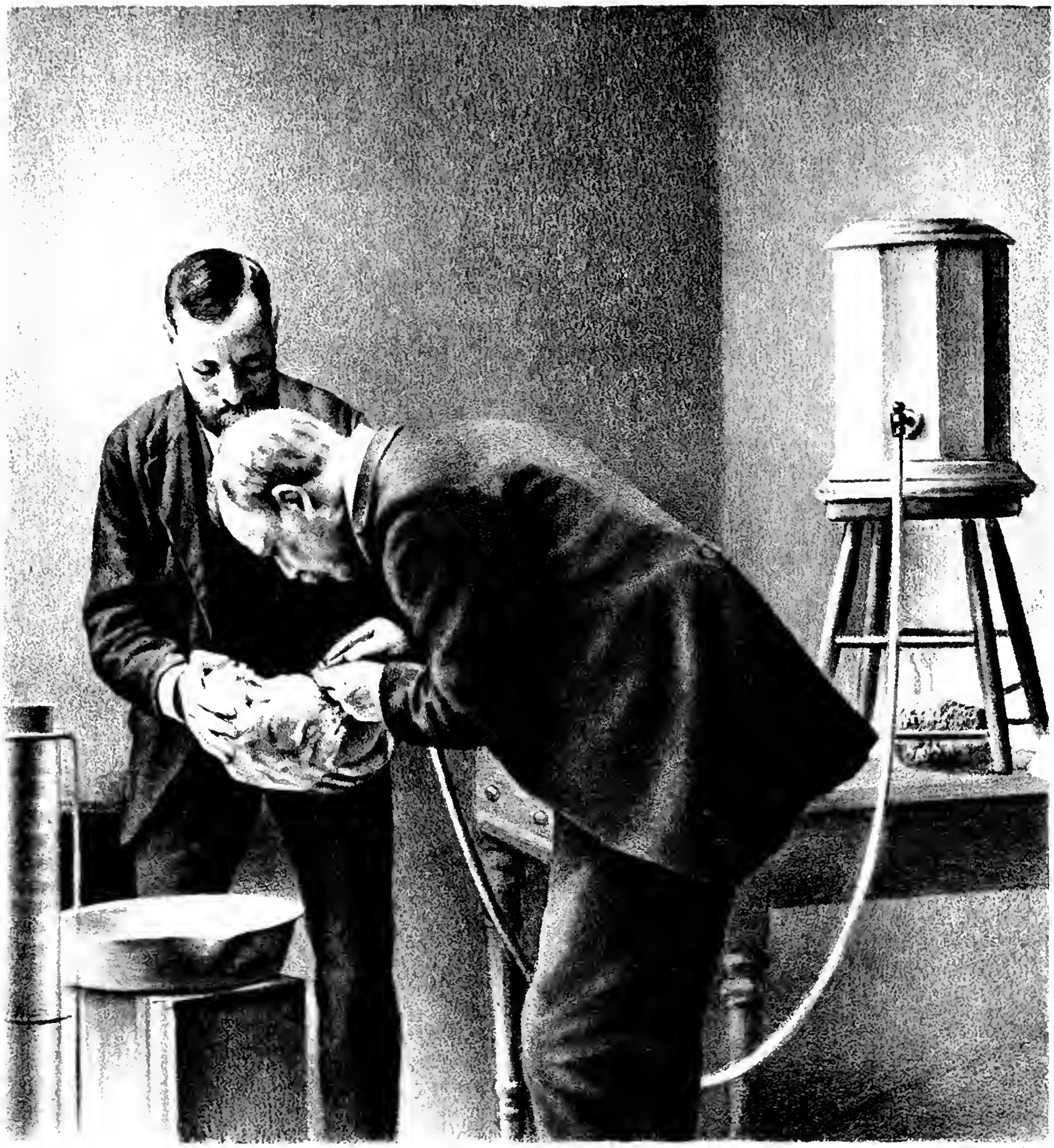
* Not tried.

TABLE IV.—*Showing amount of water held in the walls of the skull after measuring.*

Museum number.	Weight before measuring.	Weight after measuring.	Difference.	
	Grams.	*Grams.*	*Grams.*	
83	561	656	95	Weighed a few minutes after measuring, before drainage was complete.
87	420	470	50	
93	482	591	109	
481	518	660	112	
1517	1067	1211	144	

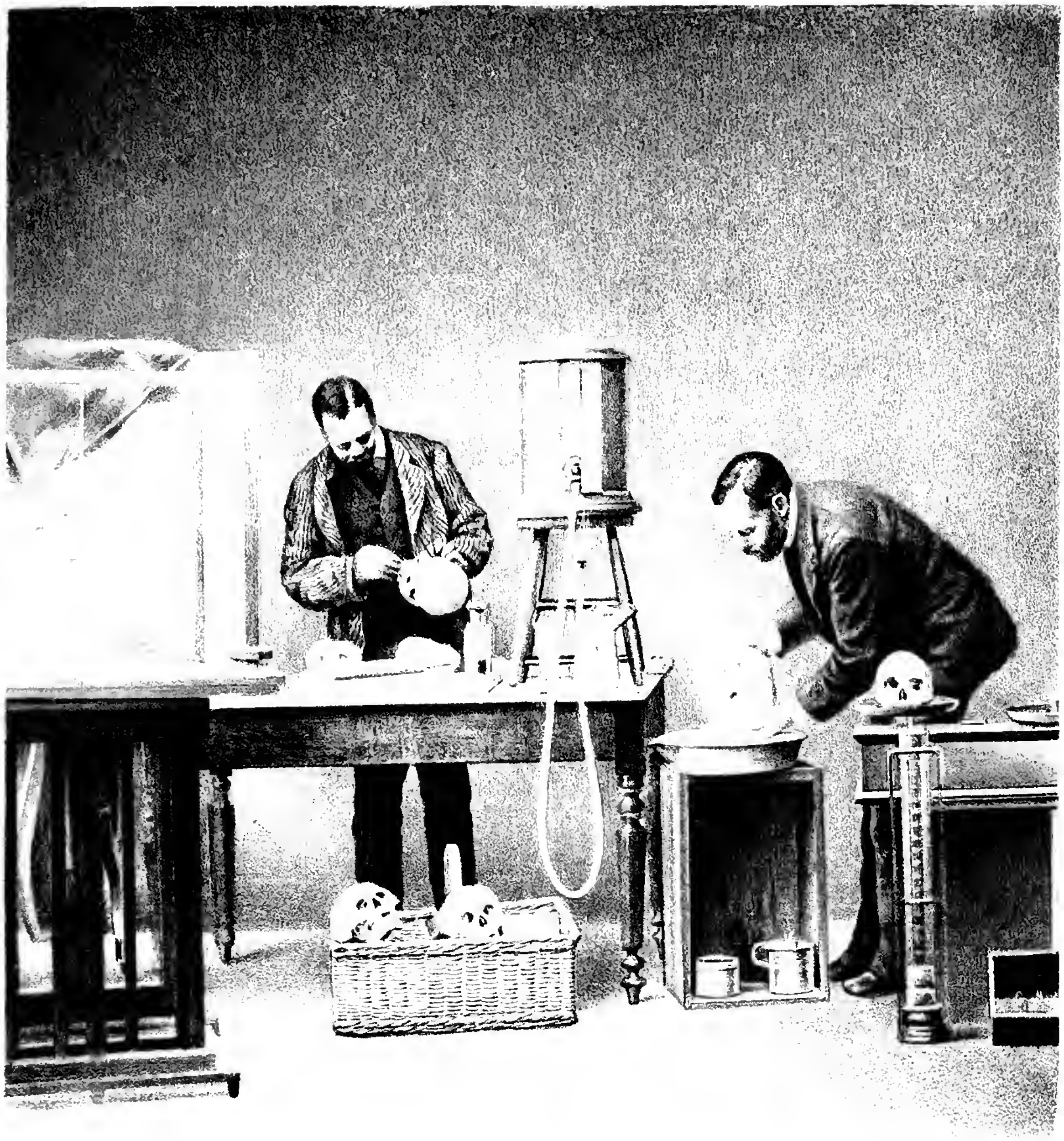
Museum number.	Weight before measuring.	Weight after measuring.	Difference.	
	Grams.	*Grams.*	*Grams.*	
84	523	561	38	Weighed several hours after measuring, when drainage was complete.
199	505	538	33	
362	452	481	29	
373	553	596	43	
375	402	456	54	

ASCERTAINING CAPACITY OF CRANIAL CAVITY
BY MEANS OF WATER



ASCERTAINING CAPACITY OF CRANIAL CAVITY
BY MEANS OF WATER

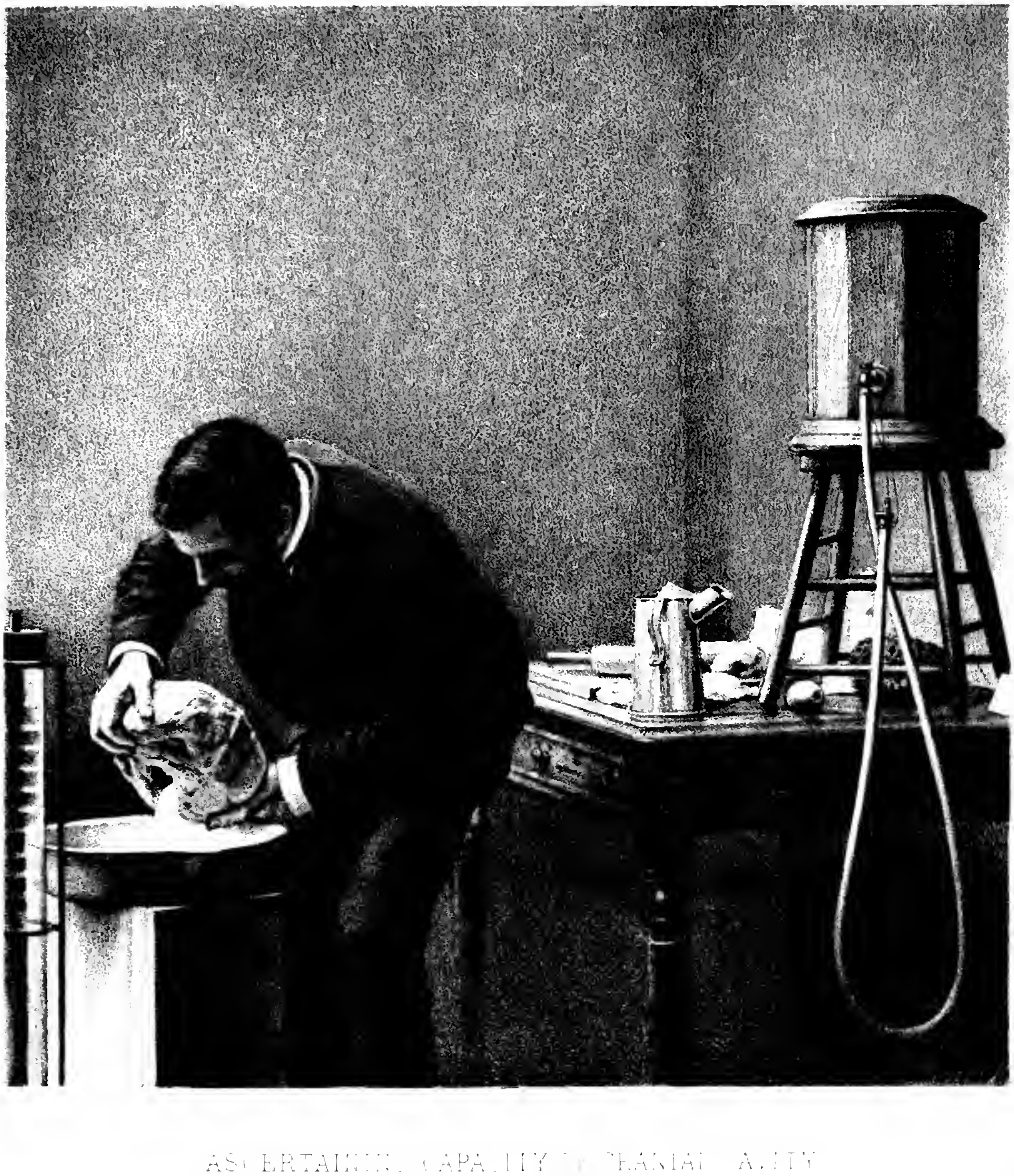
[illegible] Skull 2nd Part. The skull [illegible] with [illegible] held in the hands of an assistant while the operator [illegible] the edge of the foramen [illegible] Before this is done the skull [illegible] the water [illegible] that [illegible]



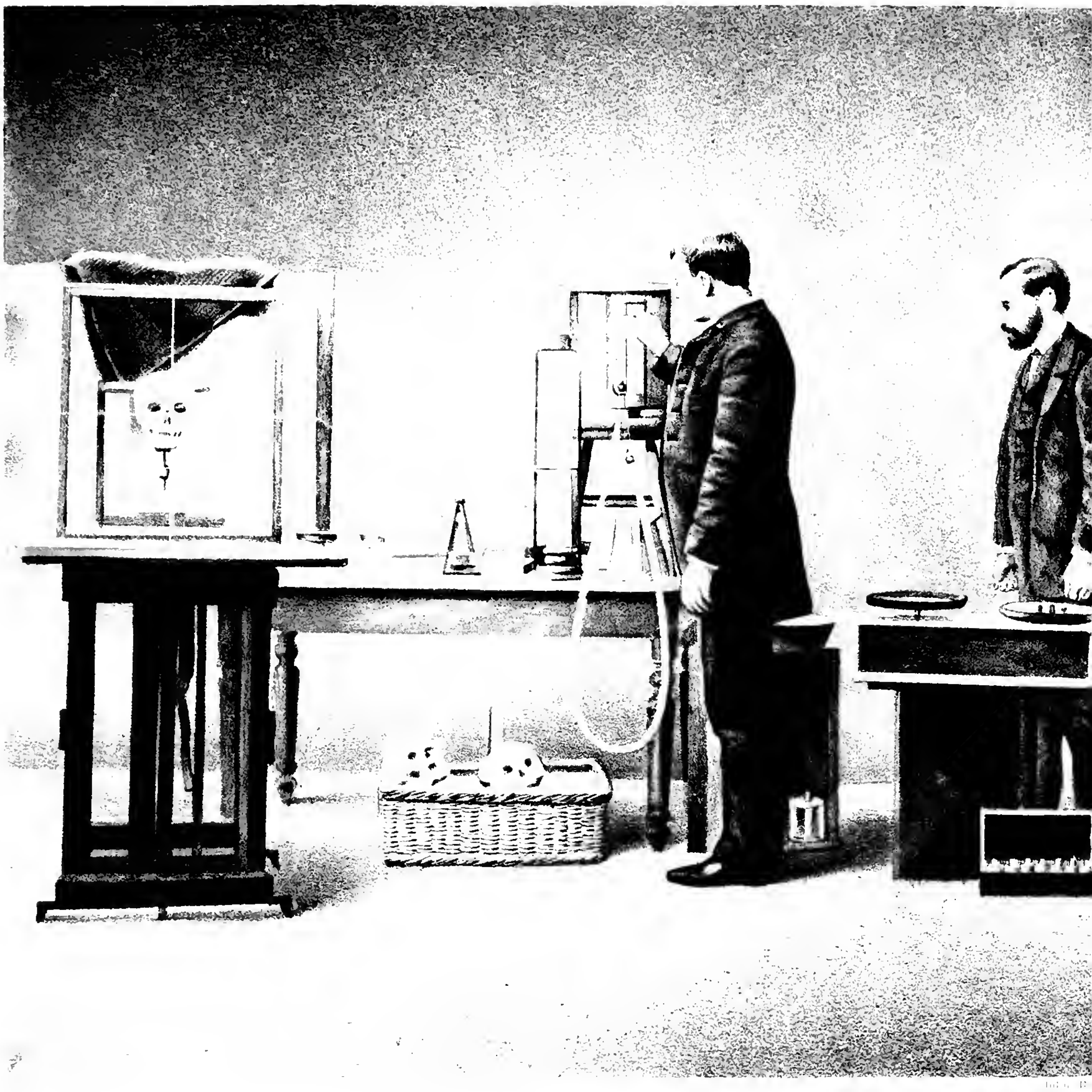
Julius Bien & Co. Lith.

ASCERTAINING CAPACITY OF CRANIAL CAVITY
BY MEANS OF WATER

PHOTOGRAPH NO. [illegible] Emptying Skull 1st Position. This picture also shows attendant removing putty from skull.



ASCERTAINING CAPACITY OF CRANIAL CAVITY
BY MEANS OF WATER.



ASCERTAINING CAPACITY OF CRANIAL CAVITY
BY MEANS OF WATER

PHOTOGRAPH 20 c. Using insufflator charged with dry powder to define the surface of the water. In this and other picture are seen various appliances: scales, weights, reservoir, tube, stop-cock, ball, gallon measure, bread board, rolling pin, measuring glass, insufflator, metronome, &c.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

FOURTEENTH MEMOIR.

ON A NEW CRANIOPHORE FOR USE IN MAKING COMPOSITE PHOTOGRAPHS OF SKULLS.

ON A NEW CRANIOPHORE FOR USE IN MAKING COMPOSITE PHOTOGRAPHS OF SKULLS.

READ NOVEMBER 12, 1885.

By John S. Billings and Washington Matthews.

At the meeting of the Academy in April, 1885, we described an extemporized contrivance for taking composite photographs of skulls, and announced that the construction of a more convenient apparatus was in contemplation. Such an apparatus has since been constructed under the direction of Dr. Matthews, and has been employed by him in taking a number of composite photographs of crania, specimens of which are herewith submitted.

The apparatus itself—of which four photographs are presented—consists of an object-stand, with four hinged frames, and a craniophore with two different attachments for holding the skull.

The object-stand is of walnut, 3 feet and 5 inches high. The top is 18 inches square and 2 inches thick, with a hole in the center through which the main screw of the craniophore descends. Frames bearing fine cross-wires are attached to the top by hinges in such a manner that they may be raised and lowered.

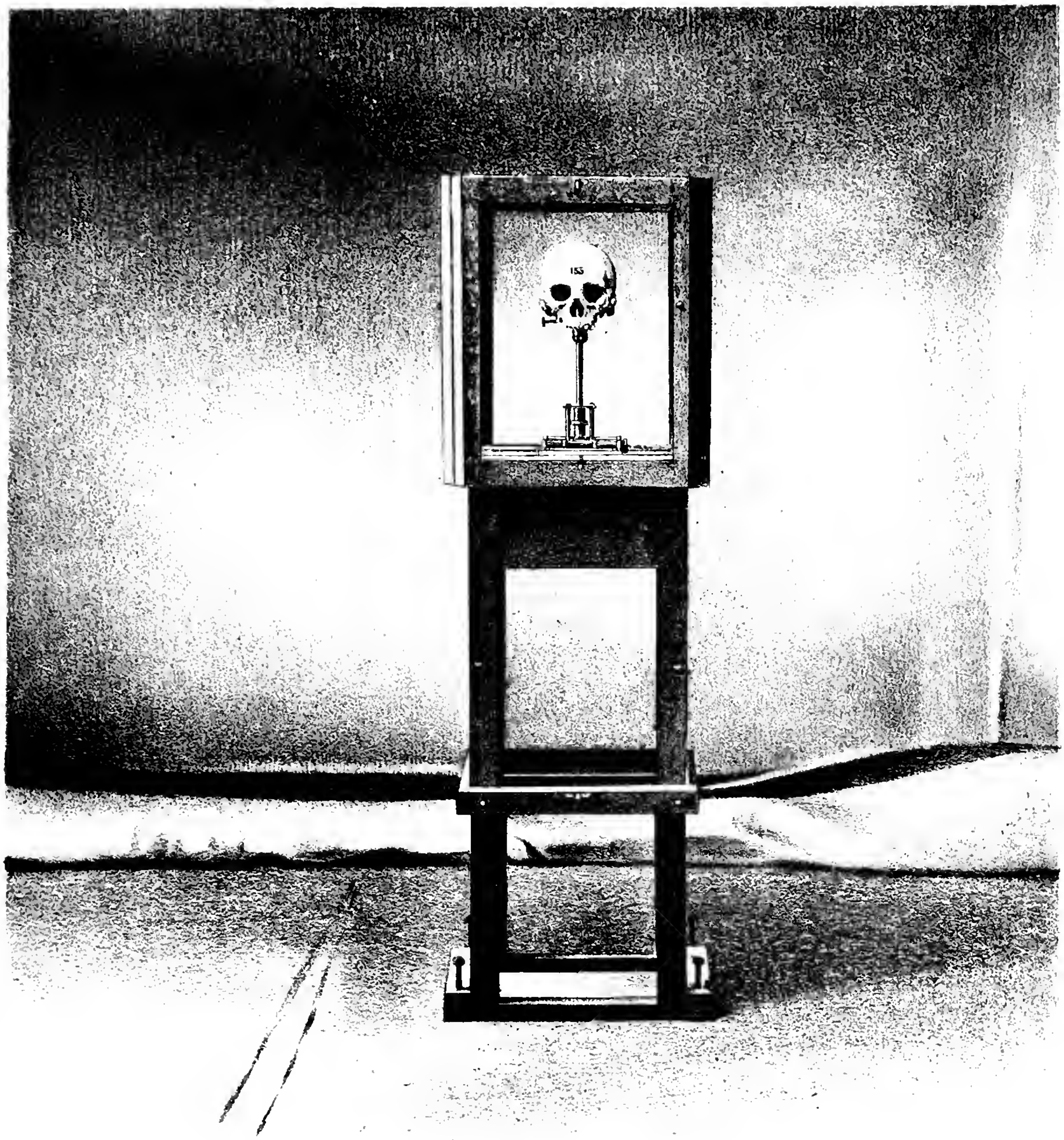
The craniophore is of brass It has a large screw to elevate and depress the skull. This screw is worked by means of a long tubular nut fixed in a frame. The latter slides on two round bars, and is moved by a smaller screw which works in nuts fixed to the bottom of the frame, and secures thereby lateral adjustment. On the summit of the screw is a ball-and-socket joint. In the top of the ball is a hole or well which receives the pin at the base of each attachment and thereby holds the latter in place.

One attachment is for supporting the skull, base downwards, when the facial, lateral, and occipital views are taken. It has a cone which enters the foramen magnum, and a jointed arm elongated telescopically, which supports the palate.

The other attachment is for holding the skull when the basal and vertical views are taken. It has two arms extending horizontally. On each of these there is a vertical bar, movable, in order that skulls of different widths may be accommodated. On each vertical bar is a short, horizontal, obtusely-pointed bar which fits into the auditory meatus and moves freely on the vertical bar. These movable parts are provided with binding-screws. The horizontal bars are attached to a plate which slides on a frame; this arrangement secures the antero-posterior adjustment necessary to insure coincidence of the selected horizontal plane with the lateral vertical wires.

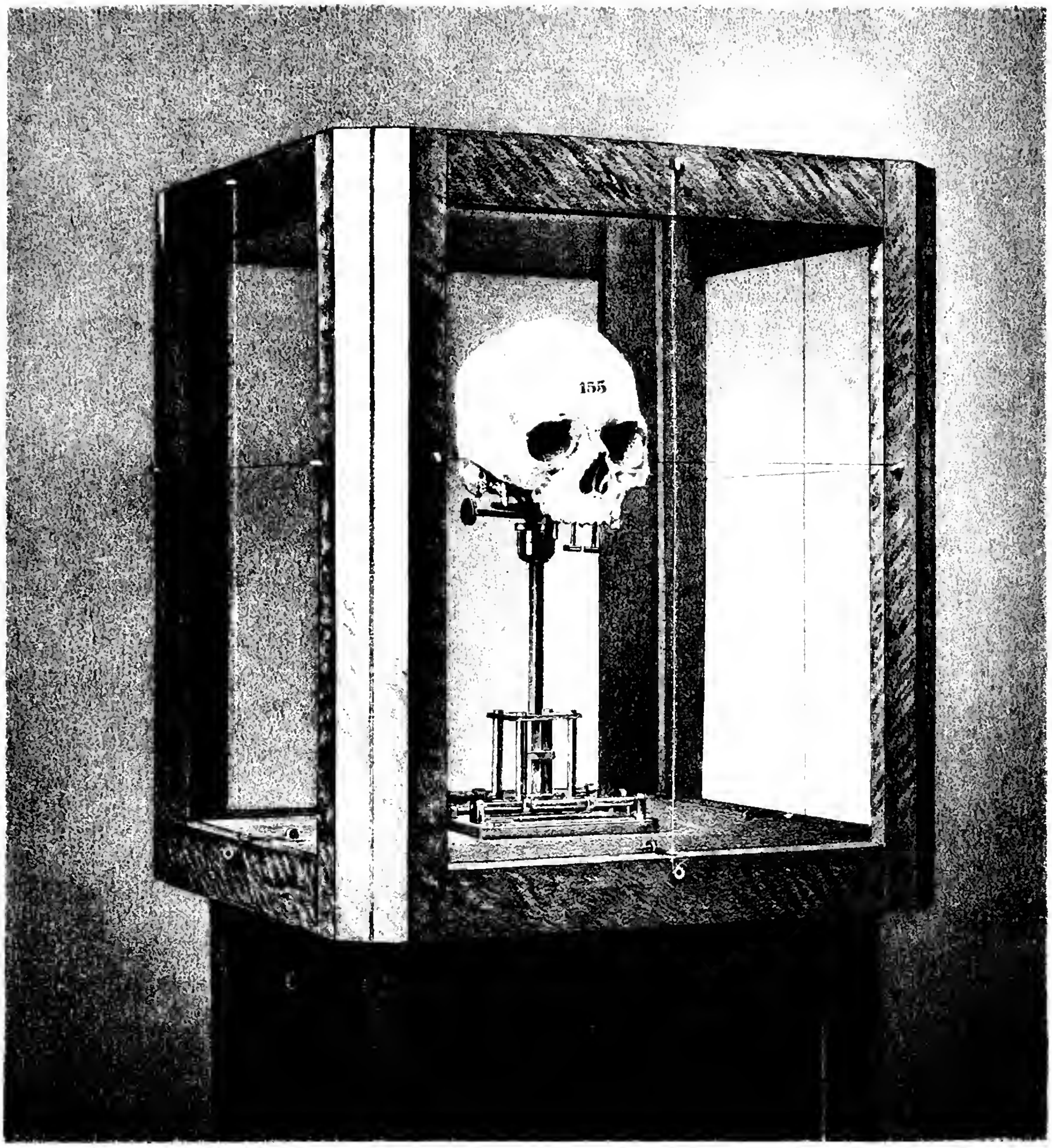
To operate: The skull is placed in the desired attachment; the latter is secured by the pin at its base to the ball in the joint. The joint is tightened by its screw to such a degree that it will move by gentle force, but not by the mere weight of the ill-poised skull. The frames are raised and maintained in their upright position by hooks fastened into eyes on the top of the table. The skull is adjusted on the four sets of cross-wires. Then the anterior frame and the lateral frame next to the window are lowered; a black velvet background is hung on the posterior frame, a large white card-board is hung on the frame further from the window, the brass-work is occluded with small velvet screens, and the picture is taken. When the work of the day is done, all the frames are folded down, fastened by buttons to the legs of the table, to secure them from injury, and the craniophore is covered.

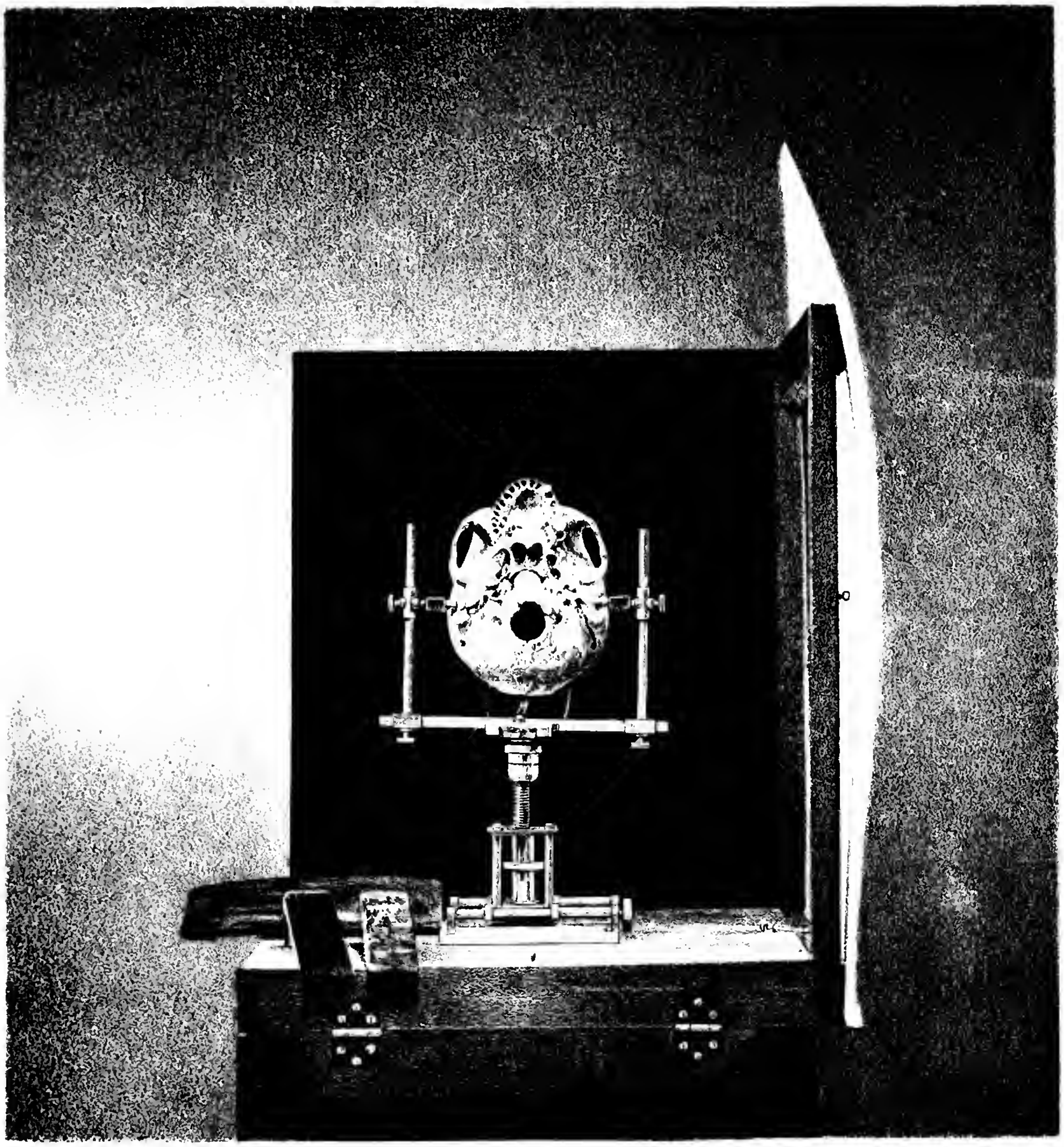
The craniophore was made by Mr. Edward Kübel, 328 First street, N. E., Washington, and cost $55. The object-stand was made by the carpenter who works at the Museum. A coarser thread for the vertical screw of the craniophore is recommended, as facilitating adjustment.

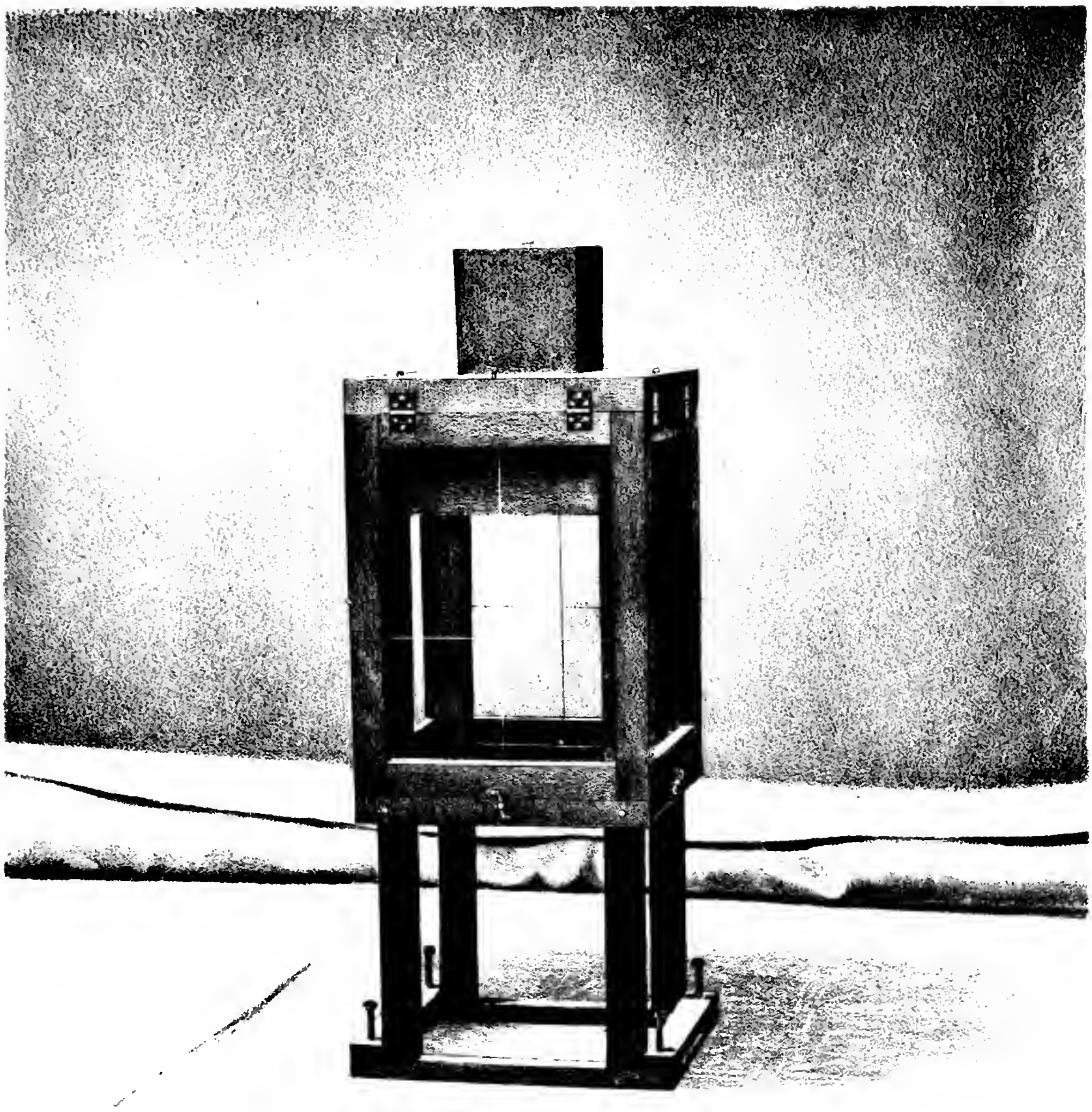


APPARATUS FOR TAKING
COMPOSITE PHOTOGRAPHS OF SKULLS

NO. 5. THE ADJUSTMENT. The frames on object-stand bearing the adjusting wires are raised. The attachment for securing skull at foramen magnum is here employed. The skull is in position for picture of norma facialis.


155





APPARATUS F[illegible]

[illegible]MPOSITE PHOTOGRAPHS [illegible]

[illegible] WHEN NOT IN USE. [illegible] are folded down and held with buttons the better to protect them. The craniophore is covered.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

FIFTEENTH MEMOIR.

ON THE SYNCARIDA, A HITHERTO UNDESCRIBED SYNTHETIC GROUP OF EXTINCT MALACOSTRACOUS CRUSTACEA.

S. Mis. 154——16

I.—ON THE SYNCARIDA, A HITHERTO UNDESCRIBED SYNTHETIC GROUP OF EXTINCT MALACOSTRACOUS CRUSTACEA. PLS. I, II.

READ APRIL 21, 1885.

BY A. S. PACKARD.

For a long time I have been desirous of examining into the relationship of the singular group of Carboniferous Crustacea represented by the genus Acanthotelson of Messrs. Meek and Worthen, as it has seemed to be a remarkable connecting link between the Edriophthalmata (or Tetradecapoda) and the Decapoda (in the older sense). An unexpected opportunity has been offered in a large series of specimens, which, without solicitation on my part, has been generously offered me by R. D. Lacoe, esq., of Pittston, Pa., and J. C. Carr, esq., of Morris, Ill. Mr. Lacoe's collection was a very rich one, comprising over forty nodules, each containing a usually well-preserved Acanthotelson. Although additional specimens are much to be desired, especially such as may show the eyes and their nature, whether sessile or stalked, a point still unknown, the eyes not having been with certainty identified, and also to better show the nature of the abdominal appendages, it seems to us that enough characters have been preserved to allow us to present a tolerably accurate account of the essential features of the group.

The genus Acanthotelson was first proposed by Messrs. Meek and Worthen, in 1860,[1] and the species described as *A. stimpsoni* M. & W. A second species, *A. eveni*, was described by the same authors in 1868.[2] Additional facts were stated and figures given in the Report of the Geological Survey of Illinois, III, Paleontology, 1868. The specimens we possess enable us to amend and to add to their original descriptions; but in doing so we wish to bear witness to the care and ability displayed by the authors in the examination and illustrations of this form. The genus is referred with doubt by the authors to the Isopoda, who also refer to its resemblance to some of the lower types of macroural Decapods. They remark: "From all the specimens of this genus now known it is evident that, in the nature of its antennæ, as well as in the forward direction of all its thoracic legs, and to some extent even in the nature of its caudal appendages, it differs from the Tetradecapoda, and approaches some of the lower types of the macroural Decapoda. In the possession of seven distinct thoracic segments, without a carapax, however, as well as in the form of all its thoracic and abdominal segments, it agrees with the Tetradecapoda, particularly with the Isopoda, which have but one pair of the abdominal appendages styliform, instead of three, as in the Amphipoda. One specimen of *A. stimpsoni* (represented by fig. B, p. 549) also appears to show the eyes (marked I in the cut) to be sessile, though remarkably prominent. If they are sessile, this must be conclusive evidence that it must be a Tetradecapod. Until other examples, showing more clearly the nature of its eyes and some other parts, can be examined, we leave it provisionally where we first placed it with doubt, in the Isopod group of the Tetradecapoda." (P. 550.)

The following description, while embracing the more general characteristics of the group to

[1] Proceedings Academy of Natural Sciences, Philadelphia.
[2] Amer. Journ. Sc., 2d ser., xlvi, 28, 1868.

which Acanthotelson belongs, also without doubt comprises the generic and specific characters. We will first give a description of the fossils themselves, based on the material we have had for examination, and then endeavor to point out those characters which we suppose to be the essential features of the group to which the genus belongs, and also to indicate the probable affinities to the other divisions or suborders of Malacostraca. It may be as well to say that, after examining some forty specimens, we are unable to distinguish between *Acanthotelson stimpsoni* and *eveni*, and are inclined to believe that the former is the young of the latter species.

In Mr. Lacoe's No. 501*b* the head is well preserved; the first arthromere or segment is considerably shorter than any of the succeeding ones; it is slightly less than two-thirds as long as the succeeding arthromere; it bears in front a well-marked, small, triangular rostrum, which is acute at the tip, and is about two-thirds as long as the segment itself; the edge of the rostrum is considerably raised, especially at the base. The front edge of the segment on each side of the rostrum is also margined with an elevated ridge. The surface of the segment is rather full and convex on each side, but not so decidedly so as the second segment. The second arthromere is about as long as those succeeding, though not quite so long as the sixth arthromere; on each side is a low boss-like swelling, situated obliquely, and prolonged in an oblique direction to the anterior outer edge. The second segment is distinctly separated by an impressed line from the first, but there is not a true articulation between them, so that the first and second cephalic segments may be said to be consolidated and to represent the carapace of the Schizopoda. The three succeeding segments have a transverse, uninterrupted, smooth ridge situated in the middle on the third, but in the fifth segment near the hind margin. The sixth and succeeding segments are smooth and even. The body is of even width to near the telson. The lower edges of the segments are evenly rounded, those of the hinder abdominal segments are more acutely rounded.

We have been unable to detect any positive traces of the eyes, nor can we state whether they were sessile or stalked, though if they were present and sessile we do not see why they should not have been preserved in some of the specimens (particularly 501^a and 406^a).[1]

The first pair of antennæ seem to arise directly from each side of the small, short, rudimentary rostrum. The scape is three-jointed, and not very long and slender; second joint not so thick, and about one-fourth shorter than the first and twice as long as thick; third joint long and slender, considerably longer than the second. The scape bears two flagella, which are long, slender, multiarticulate branches of unequal length, of which the inner is the thicker and shorter, the outer flagellum much slenderer and longer, the entire length of the antennæ being one-half that of the second or outer pair. The second pair of antennæ have also a three-jointed scape (which is not accurately represented in Meek and Worthen's figure). The basal joint is short; second joint shorter than the first, with two unequal internal spines; third joint slightly longer than the second and much smaller; there are traces of a small antennal scale; the flagellum is long and slender, its entire length about half that of the body.

There are twelve pairs of feet (506^a,^{c}), a pair to each segment situated between the head and penultimate uromere or abdominal segment; these, with the caudal pair of appendages, make in all thirteen pairs of legs.

The number of arthromeres or body-segments is sixteen, counting the head as consisting of two when seen from above, and the telson as a rudimentary arthromere, so that there are thirteen arthromeres between the head and telson, each of them bearing legs. There is no apparent distinction, as regards the segments themselves, into cephalothorax and abdomen (urosome), but there are two cephalic, nine thoracic segments, and seven abdominal, counting the telson as the seventh. The first seven pairs of (thoracic) legs are much alike in appearance, reminding us of those of Petalophthalmus and Gnathophausia; these are succeeded by five pairs of abdominal appendages, which are about half as long and large as the thoracic legs. The first pair of thoracic legs (which do not seem to be mandibular palpi) are considerably larger (broader and longer) than the succeeding ones. It is composed of six joints; the first and second rather narrow; the third broad, with, according to Meek and Worthen, "three" spines on the "under side" (these were not to be seen in my specimens, though undoubtedly existing there); fourth longer than the third, with three spines;

[1] Before going to press I received from Mr. Lacoe a very large specimen, his No. χ^4, in which are two large smooth concavities, one on each side of the base of the head; it is possible that these are sessile eyes.

fifth joint thicker than the fourth, thickening towards the distal end, with four spines, the fourth spine the largest and as long as the joint is thick; the sixth about two-thirds as thick as the fifth, with two remote spines on the under side and ending in two spines, one of them very large and stout (there is possibly a third small spine). In Meek and Worthen's figures the spines are erroneously drawn on the outer side of four joints; we find that the spines are situated only on the two penultimate joints; the terminal claw is not represented by Meek and Worthen. The succeeding six pairs are all about the same size and length, being large, well developed, long, and slender, about one half to two-thirds as thick as the first pair (406[b]), with no traces of a gill; the second pair are a little stouter than the others and apparently spined on the penultimate joint; the seventh pair the slenderest and nearly as long as the first pair; the three basal joints are long and slender, the third very distinct, long, and slender; fourth joint long, slightly swollen in the middle; fifth equal to the sixth in length, but slender, slightly thickened towards the distal end; the sixth somewhat longer than the fifth, ending in a point; none of the terminal joints appear to be chelate.

The abdominal appendages are distinctly biramous and schizopodal in their appearance. Each apparently consists of a small, narrow, jointed limb and a larger exopoditai branch (or gill(?); see 406[a,b]). We can see traces of the first two pairs. In another specimen (501[a,b]) the first three pairs of abdominal legs are to be plainly seen; the exopodital or respiratory and swimming ramus is sessile, lanceolate-oval, and broad, thickened on the hinder (?) edge. In Mr. Carr's specimen No. 1 are distinct traces of a biramous appendage on the fourteenth and fifteenth (penultimate) segments; and in his No. 3 there are to be seen the traces of the second–fourth pairs of abdominal feet, with double rami, the hinder ramus the smaller and narrower. In an abdominal foot (in Lacoe's No. 406[f,g]) the second joint is narrow, lanceolate-oval, rounded at the tip, from which arise a series of long slender setæ, about twelve in number, which form an oar-like appendage equaling in size the basal joint; total length of the limb 14.5^{mm} (the basal joint 8^{mm}, the row of setæ 6.5^{mm}=14.5^{mm}). These legs remind us somewhat of those of Squilla, as do the first thoracic pair, from their being larger than the others and armed on the under side with stout spines.

The telson is very long and slender, narrow, acute, the end very slender, with long setæ on each end; it is a little longer than the caudal feet (uropoda) on each side of it. The caudal feet, or sixth pair of uropoda, are divided into two long, large, acute rami (endopodite and exopodite) arising from a small, short basal joint (Carr's No. 1). The two rami are of nearly the same size and length, both edges of each branch being setose (the setæ are not so numerous and close as represented in Meek and Worthen's figure).

Of forty specimens examined, the total length of the largest example, including the caudal appendages, but not including the antennæ, was 75^{mm} (Lacoe's No. 58^{mm}); another still larger (No. χ^4) was 85^{mm} in length; a specimen received from Mr. Carr was 58^{mm} in length.

In a specimen of *A. eveni*, 45^{mm} in length, I made the following measurements: Width of the body, 6–7^{mm} (in Lacoe's 501[b]: Width of first cephalic segment, 5.5^{mm}; of second segment, 6^{mm}; length of first and second head-segments together, 6^{mm}; length of rostrum, 1^{mm}; length of sixth segment, 3.5^{mm}); length of first antennæ, about 12^{mm}; length of second antennæ, 26^{mm}; length of first pair of feet, 20^{mm}; greatest width of fifth joint of first feet, 2^{mm}; length of abdominal feet, 18–19^{mm}; length of telson, 13^{mm}; length of caudal appendages, 12^{mm}.

Many of the specimens are preserved flattened out, showing the back, with the legs spread out symmetrically on each side; others are preserved lying on their side, with the body somewhat arched, and then they present a shrimp-like appearance, though on a superficial examination reminding one of an Amphipod lying on its side.

The foregoing remarks apply to the larger specimens described by Meek and Worthen as *Acanthotelson eveni.* I cannot with certainty point out any distinctions from *A. stimpsoni* M. & W., the first-described species; the smaller specimens, which might be referred to the latter species, are evidently the young of *A. eveni* M. and W. Hence the specific name should be *Stimpsoni.*

The characters of this Crustacean are such as to forbid our referring it to any known group; we therefore suggest that it forms the type of a suborder of thoracostracous Crustacea, which we would designate as the Syncarida.

What we should regard as the differential characters of the group Syncarida, to which Acantho-

telson belongs, are the sixteen free segments of the body, which are homonomous or of uniform size, the first and second, however, being soldered together; the absence of a true carapace; the seven pairs of schizopod-like legs, the first pair spined and raptorial, slightly reminding one of those of Squilla; the second pair also spined; the antennæ of both pairs are long and slender, the two flagella of the first pair being very unlike any sessile-eyed or edriophthalmatous Crustacean; the six pairs of abdominal feet, which are long, slender, and with a general resemblance to those of the Schizopoda; the broader, oar-like swimming ramus, ending in long setæ. Any doubts as to the macrouran affinities of the Syncarida are removed by an examination of the long, acute telson and last pair of abdominal appendages; the appendages are biramous, the divisions flattened from above downwards, so that they with the telson serve, as in schizopods and shrimps, for propelling the body backwards when the animal is disturbed.

We should regard the Syncarida as the lowest group or suborder of Thoracostraca, but much nearer the Schizopoda than the Cumacea; they form a connecting link between the Amphipoda and Thoracostraca, but at the same time in their most essential characters stand much nearer to the Schizopoda than the Amphipoda; the lack of a carapace, even a rudimentary one, and the homonomous segmentation, causing them to bear a resemblance to the Edriophthalma, which they would not otherwise present. The Syncarida may be regarded as the homotoxial equivalents of the Decapoda, Schizopoda, or Stomapoda. To the Isopoda, Acanthotelson presents a superficial resemblance, due to the slightly vertically compressed body and the homonomous segmentation. The Edriophthalma (Arthrostraca of some late authors) are defined by Claus as "Malacostraca with lateral sessile eyes, usually with seven, more rarely with six or fewer separate thoracic segments, and the same number of pairs of legs, without a carapace," but this definition does not express those differences in the form of the antennæ, the thoracic legs, and abdominal appendages, especially those of the end of the urosome or abdomen, which are characteristic of the sessile-eyed Crustacea as distinguished from the Thoracostraca.

From the Isopoda, in which the body is usually broad and vertically flattened, with seven free thoracic segments, while the abdominal legs are lamellar and closely appressed to the short abdomen, our Acanthoteson plainly differs in the long bi-flagellate Decapod-like first antennæ, in the long homonomous segments of the abdomen, and the schizopodal abdominal feet, and especially the Schizopod-like telson and last pair of feet, adapted, as in the shrimps, for striking the water from above downwards.

The Amphipoda are, in general, characterized by their laterally compressed body, with lamellate gills on the thoracic feet, and an elongated abdomen, of which the three anterior segments bear the swimming feet, while the three posterior bear posteriorly-directed feet, adapted for springing (Claus). Now, if Acanthotelson is not an Isopod, still less should it be regarded as related to the Amphipoda. The first antennæ are entirely unlike those of any known Amphipods, the latter having a very short accessory flagellum; the second antennæ of Acanthotelson are strictly decapodous in appearance and very different from those of the Amphipoda, whereas in Gammarus the scape is as long as the flabellum. Although there are seven free thoracic segments in Acanthotelson as well as in Gammarus and other Amphipoda, those of Acanthotelson are not compressed any more than in the Schizopoda, and there are no traces of epimera; on the contrary, the free edges of the thoracic and abdominal segments are much as in the Schizopoda and Caridea. The thoracic appendages of Acanthotelson are, on the whole, like those of the Stomapoda and Schizopoda. We cannot detect any traces of mouth-parts, mandibles with their palpus, or maxillæ; but the thoracic legs do not present any close resemblance to those of the Amphipoda, the first pair being as much, if not more, like those of Squilla than any Amphipod with which we are acquainted, while the three posterior pairs, which are in form and size like those in front, entirely differ from those of Gammarus and most other normal Amphipods, in which the basal joint is very large and triangular. Turning to the abdomen, the difference in that of Acanthotelson from that of the Amphipods is still more marked. The first five pairs of uropoda, or abdominal appendages, are, in Acanthotelson, all formed apparently on the same plan, not essentially different from those of Schizopods, while the last pair are flat and on the same plane as the telson and intimately associated with the latter; in short, these parts are formed on a truly macrurous plan and most approach those of the Schizopods, in which the telson and rami of the last pair of feet are narrow and more

or less acute at the end. There is nothing in the structure of the urosome and its uropoda in Acanthotelson to remind us of the same parts in the Amphipoda.

Excluded from the sessile-eyed Crustacea, and forced to place Acanthotelson in the Thoracostraca, we are confronted by the lack of a carapace and the homonomous segmentation of the body. These are essential fundamental characters, but still the nature of the appendages and telson is such as to forbid us from rejecting the Syncarida from the ordinal limits of the Thoracostraca. We are compelled, therefore, to regard the group as a suborder standing near or at the base of the Thoracostraca, not far from the Stomapoda and Schizopoda, and with appendages closely homologous with those of these two groups. The Syncarida, from their lack of a carapace, and from the well-formed dorsal arch of the seven thoracic segments, we are obliged to consider as an annectant or synthetic group, pointing to the existence of some extinct group which may have still more closely connected the sessile-eyed and stalked-eyed Crustacea.

Notice of Acanthotelson ? magister (n. sp.).

Pl. II, Figs. 4, 5.

I have received from Mr. J. C. Carr, for examination, a specimen from Mazon Creek, collected at the same place as the nodules containing the Acanthotelson, showing the remains of a crustacean closely simlar to, if not generically identical with Acanthotelson. Unfortunately the head and antennæ are not preserved sufficiently well for description, so that the following account should be regarded as provisional, until better-preserved specimens are found. As seen by the photograph (Pl. II, figs. 4, 5), the animal was of the same general shape as in Acanthotelson; when it died the body was curved on itself, so that the two longer antennæ crossed the end of the abdomen with its appendages. The abdomen in its dorsal aspect, with the telson and last pair of uropoda, are tolerably well preserved. The faint traces of the head, unless we are mistaken, show that it was of the same general shape as in Acanthotelson. There are traces of two pairs of antennæ; one fragment, the innermost, showing traces of six joints; and there are faint impressions, not showing the joints, of two long antennæ, which are about half as long as the body. There are no traces of any thoracic or abdominal appendages except the last pair of uropoda.

Description.—Body very broad, being nearly twice as broad as the largest *Acanthotelson eveni*, M. & W. The penultimate abdominal segment is a little more than one-half as long as the terminal segment. The last segment is very large and square, the sides nearly even, not narrowing posteriorly, and it is the broad square shape of this segment which will readily enable one to separate it from the previously described species of Acanthotelson. The telson is stout, broad at the base, and rather short, much shorter than the uropoda appended to the same segment. The terminal uropoda are broad and stout, with no traces of setæ. The basal joint is broad, triangular, but a little longer than broad; the outer ramus is of moderate length, ensiform, and slightly longer than the telson; there is only a fragment of the inner telson left in the fossil, which, however, shows that it was considerably narrower and smaller than the outer pair.

Probable length of the whole body, not including the antennæ or telson, 70^{mm}.

Length of penultimate abdominal segment, 5^{mm}.
Breadth of penultimate abdominal segment, 12^{mm}.
Length of terminal abdominal segment, 10^{mm}.
Breadth of terminal abdominal segment, 11^{mm}.
Length of telson, 10^{mm}; breadth at base, 2^{mm}.
Length of basal joint of last pair of uropoda, 4^{mm}; breadth, 3.5^{mm}.
Length of outer ramus of last uropod, 11^{mm}; breadth, 2^{mm}.

Explanation of Plate I.

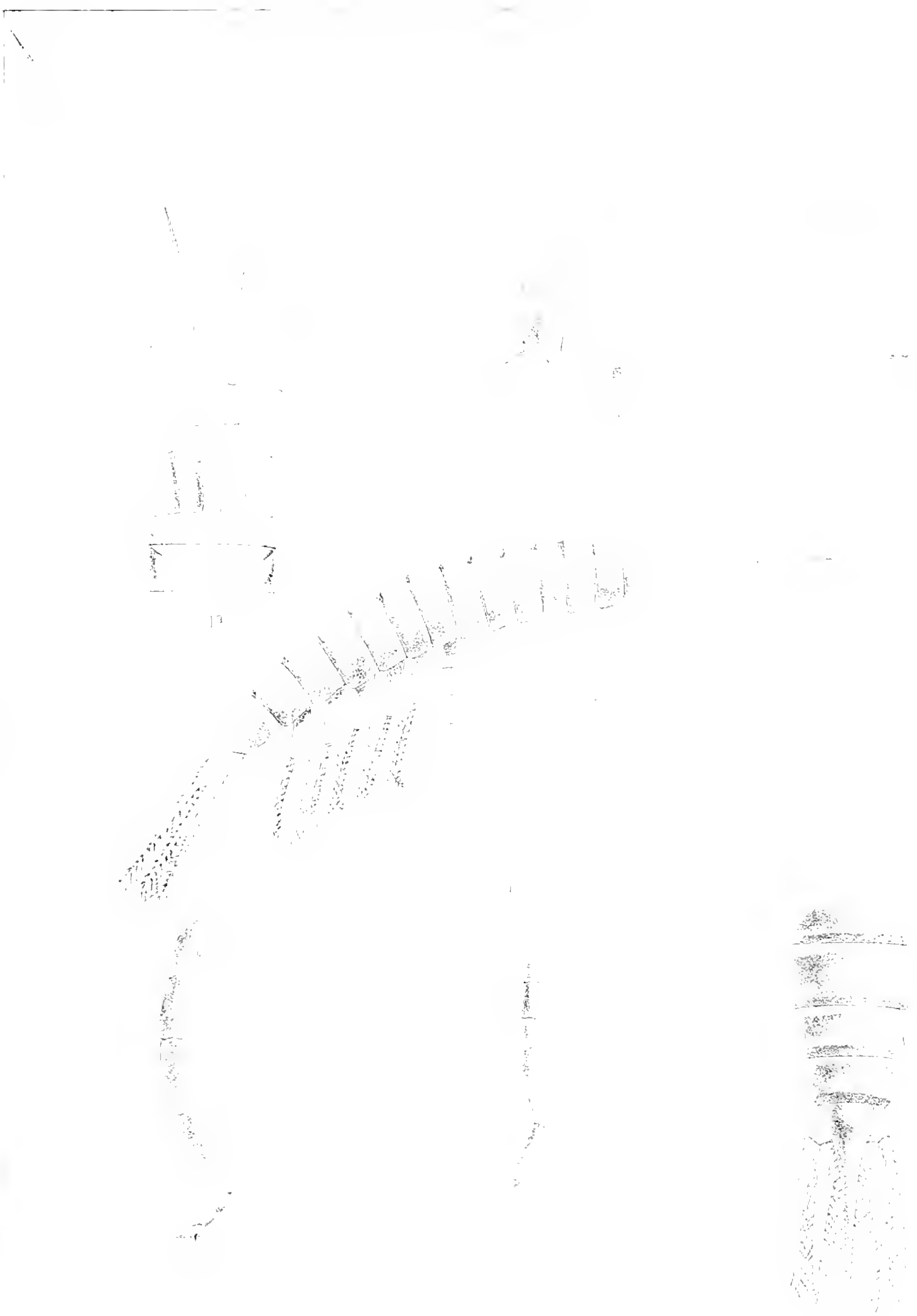
Fig. 1. *Acanthotelson stimpsoni* M. & W., restored, enlarged twice.
Fig. 1*a*. *Acanthotelson stimpsoni* M. & W., head and antennæ seen from above, enlarged twice.
Fig. 1*b*. *Acanthotelson stimpsoni* M. & W., first thoracic leg $\times\frac{3}{1}$.
Fig. 1*c*. *Acanthotelson stimpsoni* M. & W., sixth thoracic leg $\times\frac{3}{1}$.
Fig. 1*d*. *Acanthotelson stimpsoni* M. & W., telson and last pair of uropoda $\times\frac{4}{1}$.
Fig. 2. *Acanthotelson ? magister* Pack., $\times\frac{2}{1}$. All the figures drawn by Dr. J. S. Kingsley.

Explanation of Plate II.

Fig. 1. *Acanthotelson stimpsoni* M. & W.
Fig. 2. *Acanthotelson stimpsoni* M. & W., reverse of fig. 1.
Fig. 3. *Acanthotelson stimpsoni* M. & W.
Fig. 4. *Acanthotelson ? magister* Pack.
Fig. 5. *Acanthotelson ? magister* Pack., reverse of fig. 4.

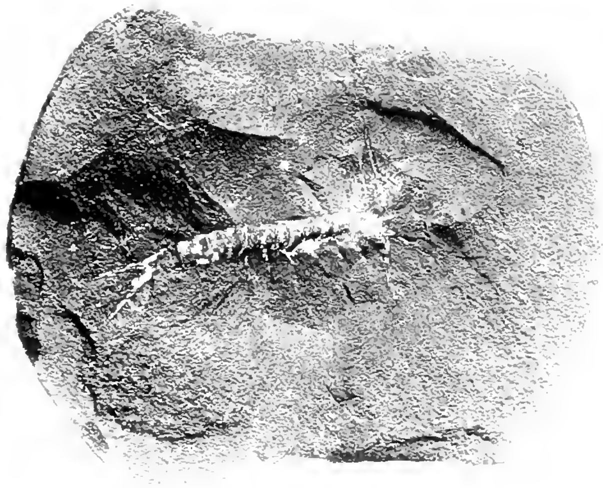
From photographs taken by Mr. Robert L. P. Mason.

Note on an additional specimen.—Since this memoir was sent to the printer I have received a larger specimen from Mr. Lacoe, labelled "Braidwood, Ill., Q[t]", which, exclusive of the antennæ and telson, measures about 82^{mm}. There are traces of four pairs of thoracic feet which are long and slender and bent backwards from the head, reminding us of the four hinder legs of an ordinary shrimp seen from one side. There are traces of the antennæ, better preserved than in the original specimen. There appear to be a pair of large antennæ, the scape composed of three large joints, the second and third smaller and together equalling in length the basal joint; these antennæ appear each to bear a large antennal scale, resembling those of the Macrura, and reaching as far as the middle of the third antennal joint. The characters shown by this specimen lead me to refer it to a genus distinct from Acanthotelson, for which the the name *Belotelson* (the entire name, *Belotelson magister*) is proposed. Additional specimens are much desired to complete our knowledge of its affinities.

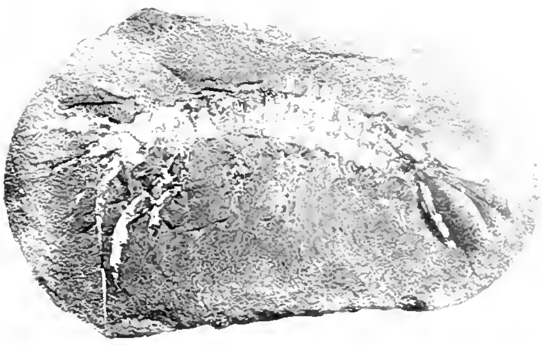


4

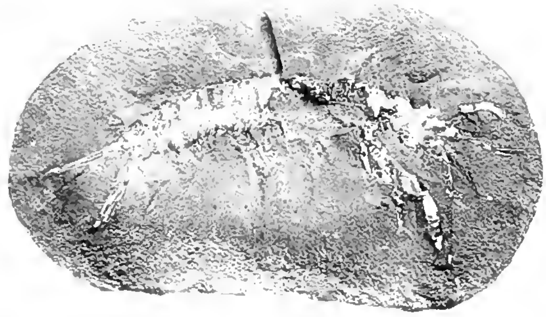
5



3



1



2

FIGS. 1–3 ACANTHOTELSON STIMPSONI. 4, 5 A? MAGISTER.

FROM PHOTOGRAPHS BY [illegible]

II.—ON THE GAMPSONYCHIDÆ, AN UNDESCRIBED FAMILY OF FOSSIL SCHIZOPOD CRUSTACEA. PL. III, FIGS. 1-4; VII, FIGS. 1, 2.

READ APRIL 21, 1855.

By A. S. Packard.

The opportunity of examining at my leisure about a dozen specimens of *Palæocaris typus* of Meek and Worthen, kindly afforded me by Messrs. R. D. Lacoe and J. C. Carr, has enabled me to work out some characters of this genus not mentioned by the original describers. The study of these specimens has induced me to compare the genus with Gampsonyx, and the result has led to the formation of a family or higher group for the genera, which should probably stand at the base of the Schizopoda, while also serving to bridge over the chasm existing between the Thoracostracous suborders, Syncarida and Schizopoda.[1]

Palæocaris was first described by Messrs. Meek and Worthen, in the Proceedings of the Academy of Natural Sciences of Philadelphia (1865, p. 48), from specimens occurring in clay-stone concretions in the lower part of the true coal measures, at Mazon Creek, Morris, Grundy County, Illinois. Afterwards, in the third volume of the Reports of the Geological Survey of Illinois, 1868, the same authors figured the fossil, and expressed themselves as follows regarding its affinities: "Hence it would seem to present something of a combination of decapod (macrouran) and tetradecapod characters. That is, it possesses the caudal appendages, anteriorly directed thoracic legs, the antennæ (some of the specimens appear, also, to show basal scales to the outer antennæ) and general aspect of a macrouran, with the distinct head, divided thorax (without a carapace), and seven pairs of thoracic legs, of a tetradecapod. We have not been able to see its eyes, but from its other decapod characters, and its analogy to Gampsonyx, which is said by von Meyer to have pedunculated, or at any rate movable, eyes, we are strongly inclined to believe that our fossil will be found to agree with Gampsonyx in this character also.

"It therefore became a matter of interest to determine to which of the subclasses, Decapoda or Tetradecapoda, it really belongs. That it belongs rather near Gampsonyx, though not to the same subordinate section (Schizopoda), there can be little doubt. Hence these two forms apparently fall naturally into the same family. Professors Jordan and von Meyer seem to have regarded Gampsonyx as a Tetradecapod, connected with the Amphipoda, but also possessing macroural decapod affinities. Professor Dana, however, regards it as a low type of Macrura, belonging to the section Schizopoda. He and Dr. Stimpson, to whom we sent sketches of our better specimens of Palæocaris, concur in the opinion, judging from all its characters yet known, that it is a low embryonic type of the Macrura, in which the carapace is not developed.

[1] We have not seen Burmeister's memoir "Ueber Gampsonychus" (Abh. d. naturf. Ges. in Halle, ii, 191, 1855), but Zittel (Handbuch der Palæontologie, p. 670) quotes Burmeister as stating that he regarded it "as the representative of a special group of Crustacea, which unites in itself some of the most essential features in the organization of the Stomapoda and Amphipoda."

"Generically, it is separated from Gampsonyx, figures of which (cuts C and D) we have added for comparison, not only in the nature of its caudal appendages, but in the more important character of having its thoracic legs simple, and not bifid, as in the Schizopoda."

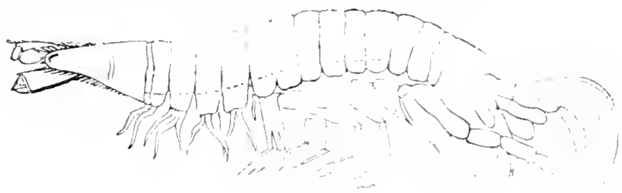


FIG. I.—Gampsonyx fimbriatus. After Jordan and von Meyer. From Meek and Worthen.

We will now describe in detail *Palæocaris typus*, restoring it so far as possible in our description from the specimens received from Messrs. Lacoe and Carr, amounting in all to about a dozen, of which ten were kindly loaned by Mr. Lacoe. Dr. Kingsley has also obligingly drawn a restoration of the fossil from the specimens sent him for the purpose. There are no traces of a carapace, but the head is plainly distinct from the rest of the body. It is rounded in front, with no traces in my specimens of a rostrum, and is apparently composed of two segments. The body, seen sidewise, is suddenly arched or bent at the articulation of the thoracic and abdominal regions, as in stomapods and shrimps, and of the usual proportions. All the segments behind the head are free, and are fourteen (seven of which are abdominal) in number, counting the telson as one. There are thus sixteen segments, the head composed of two, the thorax of seven, and the abdomen of seven. The body thus has apparently the same number of thoracic and abdominal segments as in the existing Stomapoda. It is probable that the head of *Palæocaris* is composed of the same number of segments as in the Schizopoda, but as the mouth parts have not been preserved, this point must remain undetermined. The thorax, in its general shape, as seen from above, is of the normal shape, as seen in existing Stomapoda. The abdomen is much narrower than the thorax, with the basal segments short, and the penultimate one longer than broad, widening out a little on the hind margin, and excavated behind to receive the base of the telson.

The first antennæ are about one-half as long as the body, with the scape long and slender, three-jointed (unless what I regard as the basal joint consists, as appearances suggest, of two); first joint long and slender; second, as thick but only one-half as long as the first; third, moderately long, considerably longer than the second; flagella nearly equal in size, long and slender.

The second antennæ with the scape three-jointed, the basal joint long; second and third, of nearly the same size and length; flagellum thick at base, long and slender, entire antenna nearly half as long as the body of the animal.

Of the thoracic feet, six pairs can be detected, while in front of the first pair are two other appendages like the legs, but whether they are gnathopods, like those of other Schizopoda, or thoracic feet, it is difficult to judge. Each thoracic foot is long and slender, the three distal joints forming the greater part of the limb. The terminal (seventh ?) joint is very long and slender, and probably ends in a single claw. The penultimate joint is about two-thirds as long as the terminal. It is thickened towards the end, and is perhaps a little shorter than the third joint from the end.

The endopodites* are distinctly preserved; those on the last four pairs of legs are long, narrow, lanceolate-oval, acute at the end, each side of the endopodites being alike, *i. e.*, one not being more convex than the other. If extended, the endopodite would reach out to near the middle of the terminal joint of the limb. I think I can detect eight pairs of endopodites—six at least—one on each thoracic leg and one on each of the gnathopods, if such they are. This would tend to show that the first two appendages behind the head are true gnathopods, like those of existing Schizopods, especially Petalophthalmus.

There are traces of a pair of abdominal legs to each of the seven segments. To the rather

* I had regarded these appendages as breeding lamellæ, but Dr. Kingsley suggests that they are endopodites, and though the joints are very indistinct, I am disposed to accept his correction, and will speak of them as endopodites. We should, on general grounds, regard them as endopodites

thick and long basal joint of each were probably attached two slender rami. The entire limbs were about one-half as long as the thoracic legs (see Lacoe's No. 404^{cf}). There were at least five pairs (and I think traces of a sixth) besides the last pair. The end of the abdomen, with the telson, and last pair of legs are as described and figured by Meek & Worthen. The telson is large in size, broad and short, somewhat triangular, being broader at the base than at the end. It is somewhat spatulate in form, being well rounded at the end, and much shorter than the inner rami of the appendages associated with it. Its end is fringed with coarse setæ. In the last abdominal appendages, the outer ramus is broader than the inner, with a deep longitudinal crease, or impressed line, which fades out on the outer third, or extends to the end of the basal joint. The second, or distal joint, is fringed with fine setæ. The suture between the two joints is externally indicated by two setæ larger than the others, and somewhat curved. The inner ramus is somewhat shorter than the outer; the end well rounded, and fringed with setæ. It reaches to the second joint of the longer outer ramus.

Total length of the largest specimen 33^{mm}.

Total length of the best preserved specimen 25^{mm} (Lacoe's No. 404^{ef}). This specimen gave us the following measurements:

Length of 1st antennæ (estimated) 8^{mm}.
Length of 2d antennæ (estimated) 10–11^{mm}.
Length of last thoracic leg (exopodite) 8^{mm}.
Length of endopodite 4^{mm}.
Length of telson 3^{mm}; width 1.5^{mm}.
Length of outer ramus of last pair of abdominal feet 4^{mm}.

It should be observed that the endopodites are in part represented in Meek and Worthen's figure, but not referred to in their description. They are also partly represented in their copy of Jordan and von Meyer's figure of *Gampsonyx fimbriatus*. In the latter, there is also present what is apparently a large, coarsely spined, mandibular palpus, somewhat like that in the male of the existing deep-sea Schizopod *Petalophthalmus armatus* described by Willemoes-Suhm.* In the females, however, the palpus is small and unarmed. In the figure of Gampsonyx referred to, the thoracic legs themselves, irrespective of the endopodites, are represented as biramous, and the two rami are drawn as of nearly equal length. It is probable that there has been a mistake in drawing the legs, as in none of the existing Schizopods, such as Mysis and its allies Euphausia, Gnathophausia, Petalopthalmus or Chalaraspis, are the legs thus thrice divided. It is to be hoped that the fossil itself will be examined anew with regard to this important point.†

It is sufficiently evident, however, that Gampsonyx and Palæocaris are closely allied forms, and as first suggested by Messrs. Meek and Worthen should fall into the same family, which may be called Gampsonychidæ. The principal character which separates this group from all other Schizopods is the entire absence of a carapace.

It is worthy of notice, however, that the size of the carapace is very variable in the Schizopods, and in the genus Petalophthalmus there is a great discrepancy in the two sexes. In the female it covers the entire thorax, while in the male it is remarkably small, subtriangular, leaving the two hinder thoracic segments entirely exposed, as well as the sides of the two segments in front. In the large size and oval-lanceolate shape of the endopodites, both of the gnathopods (maxillipedes) and thoracic feet, the Gampsonychidæ agree with Petalopthalmus, in which they are large and broad. In the shape of the telson and the comparative size and proportions of the last pair of abdominal appendages there is a close relationship in the Gampsonychidæ to the Schizopod genera Petalophthalmus and Chalaraspis, especially the latter genus, in which the telson is rounded at the end,

* On some Atlantic Crustacea from the Challenger Expedition, by Dr. R. von Willemoes-Suhm. Linnæan Transactions. Zoology, vol. i, p. 23, 1874.

† No light is thrown on the nature of the limbs by the thirty specimens of *Palæocaris scoticus* described by Mr. B. N. Peach from the lower Carboniferous rocks of Scotland. Nor were eyes with certainty detected in his specimens. "For instance, although in most of the specimens there occur small oblong bosses just in the place where their eyes should be, were they decapods, figs. 10–10*d*, yet the facets of the cornea have been looked for in vain. This is unfortunate, as it prevents one from saying with certainty that these are the eyes, though there is a strong presumption in favor of their being so. No sessile eyes have been observed on the carapace, neither has a trace of anything been observed that could be construed into such."—Trans. Roy. Soc. Edinburgh, 1882, p. 86.

while the two rami are more as in Petalophthalmus, though broader. The other biramous abdominal appendages in the Gampsonychidæ are truly schizopodal.

FIG. 3.—Petalolphthalmus armatus♂.

FIG. 3a.—Telson of Petalophthlamus♂.

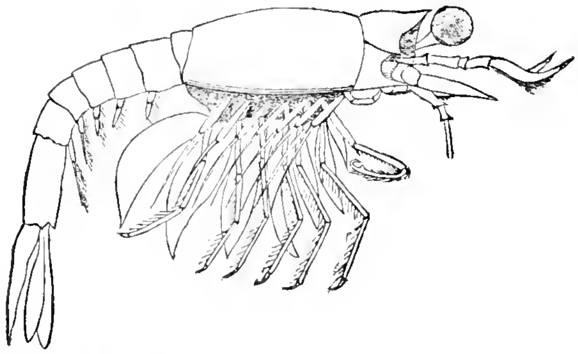


FIG. 4.—Petalophthalmus armatus♀. This and Fig. 3 after W. Suhm.

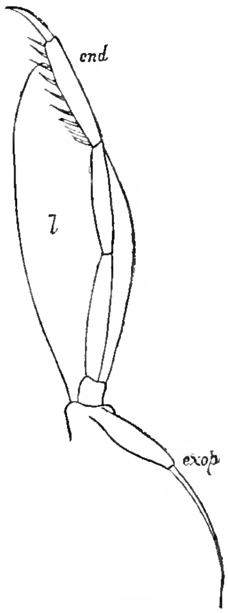


FIG. 4a.—Second gnathopod of Petalophthalmus ♀. *l*, breeding lamella.

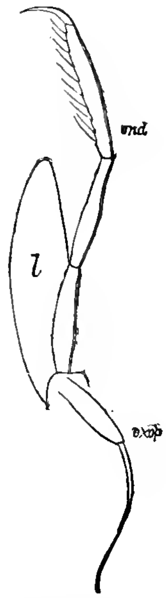


FIG. 4b.—Third pereiopod of Petalophthalmus♀.

Classifying the Schizopoda by the carapace, modifying Willemoes-Suhm's table by throwing out the Nebaliadæ and substituting the Gampsonychidæ, there would seem to be three groups, as follows:

I. Carapace absent .... (Gampsonychidæ).
II. Carapace free, varying in size .... (Gnathophausia, Petalopthalmus and Chalaraspis).
III. Carapace fastened to the thorax .... (Mysis, Lophogaster and Euphausia).

But I should agree with Willemoes-Suhm that this is not a natural genealogical classification, and throwing out the Nebaliadæ, which, as we have endeavored to show, belong to a distinct order of Crustacea, the families of Schizopods may be enumerated thus (after adding the Gampsonychidæ to von Suhm's table), all having seven abdominal segments:

Carapace absent, six pairs of thoracic legs .... I Gampsonychidæ.
Carapace well developed, six pairs of thoracic legs .... II Mysidæ.
Carapace well developed, eight pairs of thoracic legs .... III Euphausiidæ.
Carapace well developed, four pairs of thoracic legs .... IV Chalaraspidæ.
Carapace well developed, seven pairs of thoracic legs .... V Lophogastridæ.

When we compare the Gampsonychidæ with the Syncarida (Acanthotelson), we see that both groups have the same number of body-segments, and that both lack a carapace; and thus, while the Gampsonychidæ are the ancestors of living Schizopods, the group as a whole probably de-

scended from Acanthotelson, which is thus a truly synthetic form, standing in an ancestral relation to all the Thoracostraca, while it also suggests that the sessile-eyed and stalked-eyed Crustacea may have had a common parentage.

*Explanation of Plate III.**

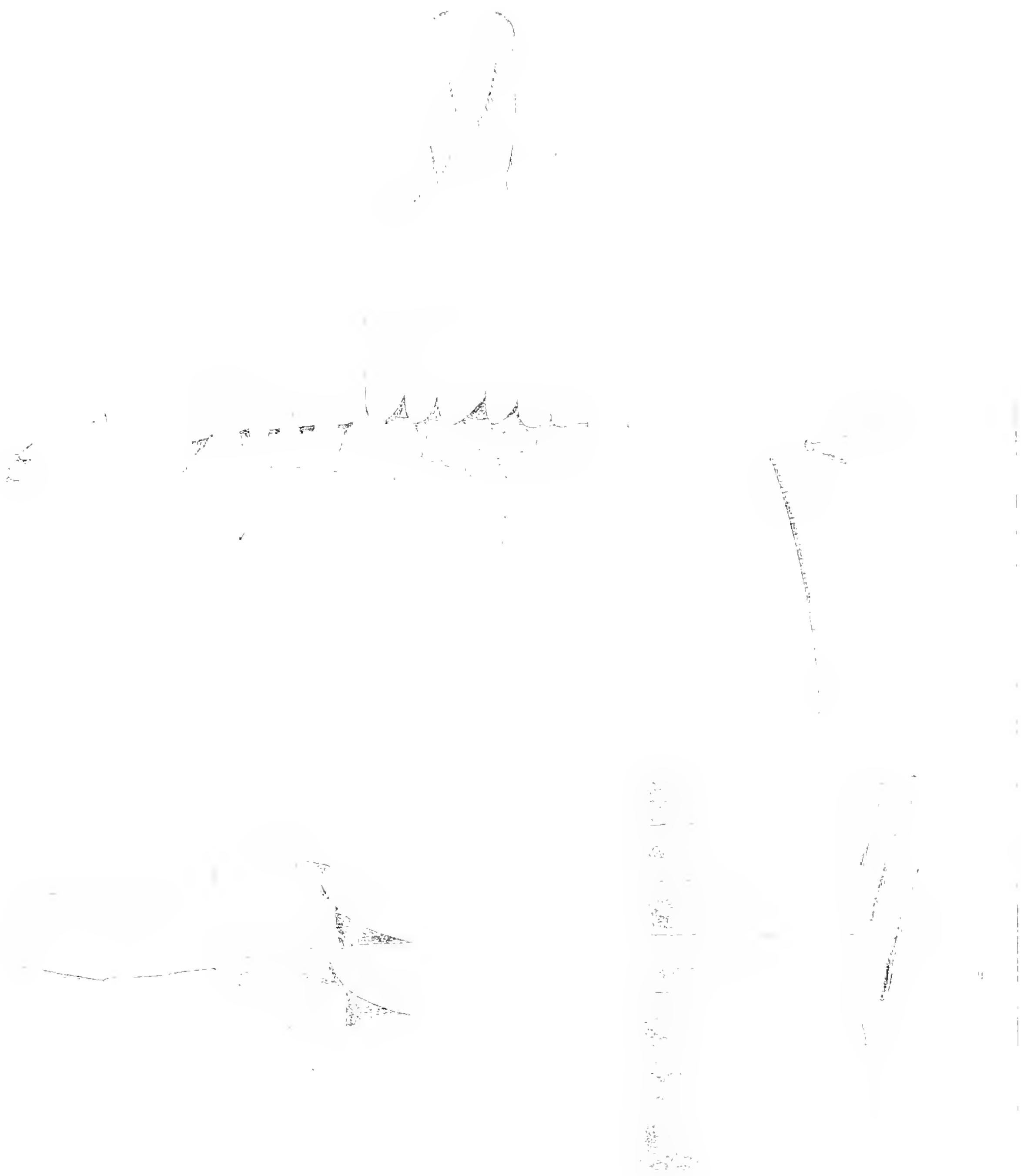
Fig. 1. *Palæocaris typus*, M. & W. restored, enlarged four times. (The front of the head is partly conjectural and though stalked eyes probably existed, no attempt has been made to restore them.)

Fig. 2. *Palæocaris typus*, seven thoracic segments, showing the disposition of the endopodites, $\times \frac{8}{1}$ (Lacoe's 404*b*).

Fig. 3. *Palæocaris typus*, dorsal view of one side of three thoracic segments, showing the basal joints of the endopodites (*endop*), and exopodites (*exop*), enlarged.

Fig. 4. *Palæocaris typus*, telson and last pair of uropoda. $\times \frac{6}{1}$.

* All the figures on this plate drawn by Dr. J. S. Kingsley.



III.—ON THE ANTHRACARIDÆ, A FAMILY OF CARBONIFEROUS MACRUROUS DECAPOD CRUSTACEA.

READ APRIL 21, 1885.

By A. S. PACKARD.

Having been kindly favored by Messrs. R. D. Lacoe and J. C. Carr with the opportunity of examining their collections of nodules from Mazon Creek containing *Anthrapalæmon gracilis* Meek and Worthen, I have been able to discover some features probably not shown in the specimens examined by Messrs. Meek and Worthen. The newly observed characters are the carapace with its rostrum, showing that the American species in these respects closely resembles the European ones figured by Salter, the founder of the genus. Moreover, our specimens prove the existence of five pairs of thoracic legs, while the antennæ of both pairs are almost entirely shown. The fact that the first pair of thoracic feet were scarcely larger than the succeeding pairs, suggests that Anthrapalæmon cannot be placed in the Eryonidæ, but should form the type of a distinct group of family rank, none of the existing Macrura, so far as we are aware, having such small anterior legs. Other characteristics which we ahall point out confirm this view.

The genus Anthrapalæmon, a Carboniferous fossil, was first described by J. W. Salter in the Quarterly Journal of the Geological Society of London (xvii, 529, 1861). The name given to the fossils has, the author remarks, "only a general signification, and is not intended to indicate a real relation to Palæmon." He also remarks that "the genus is not to be confounded with any of the Liassic or Oolitic ones published by von Meyer, Münster, &c. . . . It is broader than the general form of the Astacidæ, or than Glyphea and its Liassic allies, but much narrower than *Eryon*." Salter's type-species is *Anthrapalæmon grossarti* Salter.* With this species the American *A. gracilis* is congeneric. A closely allied English form, *A. dubius* Prestwich, is referred by Mr. Salter to the subgenus *Palæocarabus*, a name even less fitting than Anthrapalæmon. Concerning the other form provisionally referred to Anthrapalemon by Mr. Salter (his Fig. 5), we will remark in a supplementary note to this article.

The only American species we have seen† is *Anthrapalæmon gracilis* Meek & Worthen, first described in the Proceedings of the Academy of Natural Sciences of Philadelphia, 1865, and redescribed and figured in the second volume of the Geological Survey of Illinois, and again in the third volume.

Mr. Salter figured the carapace and rostrum, as well as the abdomen of the European species; while the specimen figured by Meek and Worthen evidently did not possess the carapace, but showed perfectly the telson and neighboring pair of abdominal appendages.

The specimens loaned us by Mr. Lacoe enable us to give a more perfect description and illustrations of this important type; and I am indebted to Dr. J. S. Kingsley for the restoration and

* In his Handbuch der Palæontologie, Zittel mentions *Pseudogalathea* Peach, from the carboniferous of Scotland. We have not yet seen Mr. Peach's article.

† Dr. J. W. Dawson has described and figured, the carapace of *Anthrapalæmon hillianum*, from the Carboniferous of Nova Scotia. Geol. Mag., iv, new ser., p. 56, fig. 1, 1877. Also figured in his Acadian Geology, 1878.

details, which he has so faithfully drawn. I am inclined to think that the body was actually broader than Dr. Kingsley has drawn it, and that the lateral spines of the carapace were visible from above; but I leave it as an open question.

The carapace is of the same length as the urosome (abdomen) or slightly longer, being from two-thirds to three-fourths as wide as it is long. It is very thin and delicate, and many specimens have none. The sides are regularly curved, and unarmed behind the middle, but on the anterior third are seven distinct, sharp lateral spines, the seventh being three times as large as the others and situated on the anterior outer angle of the carapace. I cannot with certainty distinguish any spines between this last-mentioned spine and the rostrum.* Casts of the latter are distinctly seen in two specimens (Lacoe's 200*pp* and 200*mm*) to be small, triangular, short, and acute. The rostrum itself is pretty well preserved in one specimen (Mr. Lacoe's No. 200*b*). It is rather long, stout, strong, acute, situated between the first antennæ, and extending as far as the middle of the third joint of the scape of the latter. In another specimen (Lacoe's 200*oo*, 200*nn*) the rostrum is fairly well preserved; it is long and slender, and about half as long as the carapace; also as long as the abdomen is wide in its narrowest part.

In only a single specimen is a side view of an apparently folded carapace preserved. The entire rostrum is long and straight, slender and acute, originating in the anterior third of the carapace, the entire rostrum being about half as long as the carapace itself. (Pl. VII, figs. 3. 3*a*.)

Along the sides are numerous sharp spines. Whether there was, as in the other form (*A. grossarti*), a series of dorsal spines our specimens do not distinctly show. Behind the base of the rostrum a median ridge extends to the posterior edge of the carapace. The lower edge of the carapace is serrate on the anterior third, as in all the other specimens. On the surface of the carapace an apparently false or superficial suture passes out laterally from the anterior third, and another impressed line, better marked, from the posterior third, extending half-way to the edge of the carapace. The surface of the carapace is seen to be finely shagreened, but scarcely tuberculated, as in the European *A. grossarti*.

Of eyes no traces are visible in any of the specimens except one, and I am inclined to the opinion that they were either wanting or very small, and concealed under the front edge of the carapace. At the same time it should be observed that in none of the fossil macrurous Crustacea from the Carboniferous are the eyes preserved. It may also be borne in mind that in the deep-sea *Pentacheles sculptus* Smith no corneal area was to be detected, and in Willemœsia and the fossil Eryoniscus the eyes are entirely wanting.† So far as we can decide, the front edge of the carapace is not excavated at the point where we should look for eyes or eye-stalks, but, on the contrary, seems to be quite regularly convex. Still, additional specimens are needed to clear up the exact nature of the front edge of the carapace.

In most of the specimens the thin, delicate carapace has not been preserved. When it is absent the five thoracic segments are distinctly marked, of about the same length. In front of these are three cephalic segments, making eight segments in all apparent in some specimens.

The first antennæ are large and long; the scape three jointed, first joint long, the second about one-half as long as the first and of about the same width; third joint a little longer, but smaller, than the second; the two flagella are a little longer than the scape, the inner one about half as thick and evidently only half as long as the outer one. (Lacoe, No. 200*ij*.)

The second antennæ are, with the scape, considerably stouter than those of the first pair; first joint short and stout, but longer than broad; second very short, oblique at the end, and considerably shorter than the third joint, which is about as long as thick; the flagellum is very long and slender, multiarticulate, at least as long as the carapace, and directed backward, as in Pentacheles; there is an antennal scale present, but its outlines are very indistinct.

The five pairs of legs are preserved (Nos. 200*pp*, 200*mm*); they are all of nearly equal size, the first pair apparently being no larger than the others, in this respect differing from Galathea and the existing Galatheidea. Of the first pair of limbs there are in one specimen (200*d*) traces of nearly

* Dr. Kingsley has, however, detected a spine at this point and inserted it in his drawings, as seen in the plate.

† After this paper was written the specimens were sent to Dr. Kingsley to be drawn; among them the specimen with traces of an eye. He has drawn in the eye; and on examining the specimen again, I think that he is right in representing the eyes. It was apparently large and well developed.

2

FIG. 5.—*Munida valida* Smith.

FIG. 6.—*Eumunida picta* Smith.

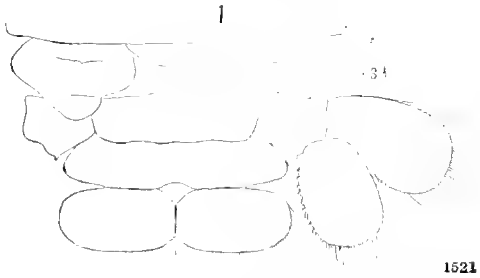


FIG. 6a.—*Eumunida picta* end of abdomen enlarged.

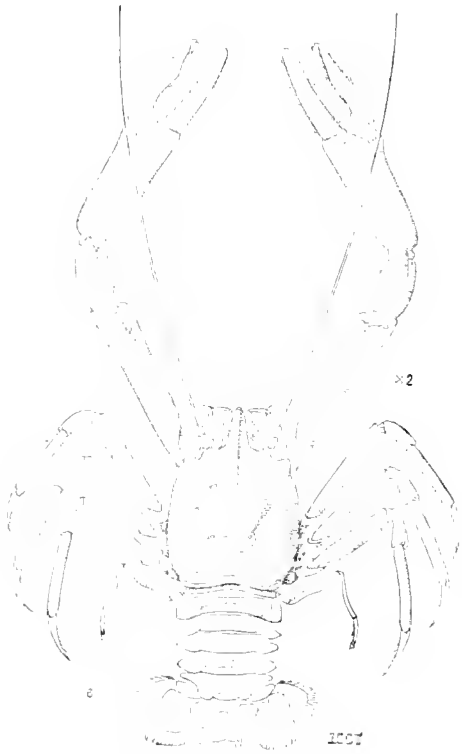


FIG. 7.—*Anoplotus politus* Smith.

RECENT DEEP-SEA GALATHEIDEA. After S. I. Smith.

S. Mis. 154—To face page 137.

the entire limb, *i. e.*, at least the first and second joints; the third joint could not have been of large size, a feature distinguishing the Eryonidæ as well as Astacidæ and the higher Macrurans in general. The first and fifth pair seem to be of about the same size; the third and fourth pair of legs are a little larger than the others and but little longer than the width of the carapace. It is unfortunate that no specimens have yet been found with the first pair of limbs entire, but the fact that the two basal and perhaps the third joints are no larger than those of the other pairs of feet indicates that this form differed from all the fossil and recent Eryonidæ, and is a character of so much importance as to forbid our regarding Anthrapalæmon as a member of that family; the only other alternative being to consider it as a type of a distinct family. Of the four hinder pairs of legs the three terminal joints of the limbs (these affording the diagnostic characters) are preserved, and the proportions are much as in the four hinder pairs of thoracic legs of the existing deep-sea Pentacheles; of the three joints the proximal and middle ones are long and slender, the inner one longer than the outer of the two; the distal (terminal) joint is rather short and pointed, and apparently chelate. Meek and Worthen remark that the legs are not divided; whether they meant that the legs are not divided as in the Schizopoda, or simply referred to the terminal joint alone, does not appear, but in the specimen before us (No. 200*pp*) the last joint appears to be chelate, since what seems to be the smaller inner finger is partly but tolerably well preserved, the crust or derm itself being preserved. Yet we may be mistaken.* In Meek and Worthen's figure, the terminal joints are drawn as undivided. If this is the case, they resemble the four hinder legs of Munida, Eumunida, and Anoplotes.

The abdomen is rather short and broad, as in the Galatheidæ, and consists of seven segments, counting the telson as the seventh.

The general appearance and relative size of the telson, together with the last pair of abdominal appendages, is much as in the Eryonidæ, with some important differences. The telson, unlike that of any other Macruran, fossil or recent, so far as I am aware, is differentiated into three portions; the basal central piece is somewhat polygonal, a little longer than broad; it is separated by a distinct suture from a small triangular terminal piece which forms the apex of the telson. Between the outer half of the entire telson and the inner ramus of the uropoda is a large broad lobe which is fringed with setæ. At first I regarded it as a subdivision of the inner lobe of the last uropoda or abdominal feet, but no instance among the Decapoda is known to us in which the last pair of uropoda have more than two lobes or divisions, and I have therefore been inclined to associate the innermost of the three setiferous lobes with the telson, and to regard the telson as divided into two median and two lateral lobular setiferous portions. Whether the two lobes belong with the telson or uropoda I will leave for the present an open question. The only group in existence in which the telson is so remarkably differentiated is the Galatheidæ. In Munida the telson is divided by sutures into four pieces, the two terminal ones lobate and edged with setæ of the same size as those of the uropoda. In Eumunida of Smith the telson is "short and broad, more or less membranaceous, and divided by a transverse articulation, so that the distal part may be folded beneath the basal part." In *Anoplotus politus*, like the foregoing, a deep-sea Galatheid, "the telson is stiffened by eight distinct calcified plates; a broad median basal plate, with a small one on either side at the base of the uropod, and a small median one behind it, and between a pair of broad lateral plates, still behind which there is a second pair, which meet in the middle line and form the tips and lateral angles." Professor Smith's figures of Munida, Eumunida, and Anopotus are here reproduced from electrotypes kindly loaned by Professor Baird, U. S. Fish Commission.†

From the nature of the differentiation of the telson in the Galatheidæ I am inclined to believe that the telson of Anthrapalæmon is subdivided in somewhat the same manner. If so, we cannot refer the genus to the Eryonidæ, and we would therefore regard it as the type of a distinct family which may thus be briefly characterized:

Family *Anthracaridæ:* Body rather broad and slightly flattened; first antennæ with two long

* In none of the six Scottish Carboniferous species of Anthrapalæmon described by Mr. B. N. Peach, do either of the thoracic limbs appear to be chelate.

† Preliminary report on the Brachyura and Anomura dredged in deep water off the south coast of New England, by the U. S. Fish Commission, in 1880. By S. I. Smith, Proc. U. S. Nat. Museum, 1883, June 18.

flagella; second antennæ long, without a scale; the first pair of thoracic legs no longer than the four succeeding pairs; the fifth pair of legs as long and well developed as the others; carapace ovate, smooth, without transverse impressed lines, with a long, acute rostrum; with lateral spines on the anterior half; abdomen rather broad, nearly as much so as the carapace; the telsŏn broad and differentiated into two median pieces, the basal piece with broad, rounded membranaceous lobes, one on each side, fringed like the two rami of each uropod, with long setæ.

After the foregoing paper was written, and an abstract published in the American Naturalist for September, 1885, I sent the specimens to Dr. Kingsley to be drawn, and on their return he made the following criticisms, which are here quoted:

"From the characters shown in the specimens before me, Anthrapalæmon apparently has nothing to do with the Eryonidæ, but belongs rather to the Schizosomi of Stimpson. The thoracic structure, antennæ, sternum, and telson are all paralleled in that group. The telson is much like that of the Porcellan crabs. The absence of the distal pedal joints of the legs renders its family uncertain. It may belong to some of those existing in the fauna of to-day. It certainly shows no features which would justify the creation of a new family for it."

While I should hardly agree with the view that Anthrapalæmon belongs to the Schizosomi, since Porcellana is a brachyuran, with a broad, round cephalothorax and small abdomen, folded beneath the body, the differentiation of the telson is somewhat as in Porcellana, as will be seen by reference to Fig. 7, copied from Milne Edwards.* On the other hand, I have erred in regarding it as closely allied to the Eryonidæ, as defined by Zittel in his Handbuch der Palaeontologie. Having already drawn attention to the highly differentiated telson of the Galatheidæ, I am now much inclined to regard the Anthracaridæ as more nearly related to this group. The resemblance to the Galatheidæ is seen in the general shape of the body, the proportions of the carapace with its sharp rostrum, and the proportions of the abdomen with its broad telson and uropoda. The first pair of antennæ differ, however, from those of the Galatheidæ in having two well-developed flagella, and the first pair of legs are much smaller, while the fifth pair are larger in proportion; the last pair of uropoda are more as in the Glyphæidæ and Astacidæ, the outer ramus being divided into a long basal and short broad distal segment.

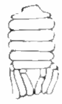


FIG. 7.—Abdomen of Porcellana.

It seems to us, from what we now know of the characters of Anthrapalæmon, as we have worked them out, that it cannot be placed in any known family of Decapoda. We should now be inclined to place the Anthracaridæ nearest the Galatheidæ, most of which are deep-sea forms. It is not improbable that they were the forerunners or ancestors of the Galatheidæ.† That the family is a synthetic group is shown by the resemblance of its telson to that of Porcellana, a Brachyuran. It certainly does not belong among the Palinuridæ, nor, on the other hand, among the Glyphæidæ.

In Zittel's valuable Handbuch der Palæontologie (Bd. 1, 2d Abth., Lief. iv, p. 682), Anthrapalæmon is placed among the Peneidæ, but its characters appear to be such as to forbid such an alliance. Palæontology is an inexact science, but the attempt to seek the natural position of extinct forms leads us to examine their remains more closely, to make further explorations for more perfectly preserved specimens, while the final result is to lead us to enlarge our conceptions as to the affinities of existing types of life. It seems to us better to establish new groups for Palæozoic forms of uncertain positions than to crowd them into groups of highly specialized modern forms. Yet this tendency may be carried too far. Whether we have erred in the present instance we leave to the judgment of those who, with a special knowledge of modern Crustacea, also possess both critical skill and broad views in dealing with natural groups.

NOTE ON THE PALÆOZOIC SHRIMPS (*Carididæ*).

The form provisionally referred to Anthrapalæmon by Salter (his fig. 5, Quart. Journ. Geol. Soc. London, xvii, 1861), occurring in the Carboniferous beds at Lanarkshire, Scotland, which has

*Crustacés, pl. 22, fig. 7.

†After writing the foregoing remarks I found I had overlooked Professor Dana's opinion, expressed on p. 350 of his Manual of Geology, 3d edition, where, after referring to the British species of Anthrapalæmon, he adds, "but the broad flattened carapax indicates a nearer relation to Æglea and Galathea than to Palæmon."

been copied into geological text-books as representing Anthrapalæmon (see Dana's Manual of Geology, fig. 686 A), does not belong to that genus or the group it represents, but is evidently one of the true shrimps or Carididæ. The carapace and serrated rostrum, as well as the shape of the abdomen, the form of the last pair of uropoda, and the telson, all indicate genuine prawn-like affinities. It may be named *Archicaris salteri*.

The other Carboniferous shrimps are *Crangopsis soliates* (Salter, Quart. Journ. Geol. Soc. 533, fig. 8, 1861). This appears to be a genuine Caridid: it is from the subcarboniferous beds of England. (As synonyms of *Crangopsis* Salter are *Palæocrangon* Salter, *non* Schauroth, and *Uronectes* Salter. (See Zittel's Palæontologie.)

Pygocephalus cooperi, of Huxley, from the Carboniferous beds near Manchester, England, is a doubtful form, which he refers "either to the decapodous or stomapodous group of the class." (Quart. Journ. Geol. Soc., xiii, 363, 1857; xviii, 420, 1862). Professor Dana (Manual of Geology, 3d edit., p. 350) regards this form as a Schizopod.

No Carboniferous Carididæ have as yet been discovered in America. The oldest known macrurous Crustacean, however, is American, the *Palæopalæmon newberryi*, described by Mr. Whitfield (Amer. Journ. Sc., 33, 1880), from the Upper Devonian of Ohio.

Explanation of Plate IV.

Fig. 1. *Anthrapalæmon gracilis*, M. & W., restored, enlarged 3 times.
2. " " " carapace and eyes, × $\frac{?}{1}$.
3. " " " carapace flattened, seen from above × 3½.
4 " " " part of first thoracic leg, × $\frac{?}{1}$.
5. " " " four basal joints of the fifth leg, × $\frac{?}{1}$.
6. " " " telson and last pair of uropoda, × $\frac{?}{1}$ to $\frac{?}{1}$.

All the figures on this plate drawn by Dr. J. S. Kingsley.

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

SIXTEENTH MEMOIR.

ON THE CARBONIFEROUS XIPHOSUROUS FAUNA OF NORTH AMERICA.

(XVI.)

ON THE CARBONIFEROUS XIPHOSUROUS FAUNA OF NORTH AMERICA.

READ NOVEMBER 13, 1885.

By A. S. Packard.

By the kindness of Messrs. R. D. Lacoe, of Pittston, Pa., and J. C. Carr, of Morris, Ill., I have been able to examine a most valuable collection of rare Xiphosuran fossils from Mazon Creek, Grundy County, Illinois, besides two specimens from the coal-beds of Pennsylvania. These have revealed the existence on this continent of two genera, hitherto confined to the European coal-measures, viz, Cyclus and Belinurus. From the Pennsylvanian coal-measures a new species of Prestwichia has been obtained, and it is probable that ultimately we shall find as many species of this family as there are in European strata.

Of still more interest is the discovery of remnants of cephalic limbs in Cyclus and Prestwichia, showing that in these animals the cephalic appendages were like those of the larval Limulus. It also appears that the ontogenetic development of Limulus is an epitome of that of the Xiphosura as a group. Furthermore, our studies have led us to restrict the Xiphosura to the three families of *Cyclidæ*, *Belinuridæ*, and *Limulidæ*, while certain upper Silurian forms referred by Woodward to the Eurypterida, and by Zittel placed among the Xiphosura, are, temporarily at least, referred to a new suborder, the *Synziphosura*, a group combining with features of its own, characteristics of the Xiphosura and some strong resemblances to the Trilobites.

Family CYCLIDÆ Packard.

Cyclus americana Packard. Pl. V, figs. 1, 1*a*; VI, figs. 4, 4*a*.

Cyclus americana Pack., Amer. Naturalist, xix, 293, March, 1885.

In a nodule from Mazon Creek, Illinois, received from Mr. Lacoe, I recognize a species of this rather obscure genus, which has not before occurred in North America, though in Europe nine species have been described.

In form the animal is perfectly orbicular, the length being exactly equaled by the breadth. The body is regularly disk-shaped, flattened hemispherical, with the edge of the body broadly and regularly expanded, the margin being thin and flat, and apparently a little wider on the sides than on the anterior or posterior end. The inner edge of the rim is separated by an impressed line from the raised portions of the body-disk; the surface of the rim is not plain and smooth, but ornamented by a series of plate-like, squarish markings, apparently separated by a slight impressed line, and with a slightly marked, raised tubercle on each plate or scale.

There are no indications of segments either of the head or abdomen, nor are the limits between a head and abdominal region distinguishable, as is the case in *Cyclus jonesianus* Woodw.* There

* Contributions to British fossil Crustacea. By Henry Woodward, F. G. S., etc. Geol. Mag., vii, No. 12, pl. xxiii, Dec., 1870.

are, however, indications of four, and perhaps five, pairs of short, thick, cephalic appendages on the anterior third of the body. Unfortunately, they are not well preserved, the basal and distal portions not present, and the indications of joints indistinct; they are directed outwards from near the median line of the body, on each side of the intestine, the hindermost (6th) pair being directed somewhat obliquely outwards and backwards. In their position and relative distance apart they seem homologous with the cephalic limbs of the larval Limulus. The indications, slight as they are, lead us to suppose that they approached in general shape and relative size those of Prestwichia, reaching near but not passing beyond the edge of the cephalic shield. The distal portion of the limbs not being preserved, it is impossible to conjecture whether they were forcipicate or not.*

Through the middle of the body, from near the anterior to the posterior margin, passes the cast of the digestive canal; it is swollen in front, the dilatation probably representing the proventriculus, and in outline the cast recalls that of the digestive canal of Limulus. Judging by analogy, the mouth was probably, as in the larval Limulus, situated well in front between the anterior pairs of appendages, and the œsophagus curved forward and upward from the mouth, while the vent was situated very near the hinder edge of the body.

There are no distinct traces of an abdominal region in the specimen, and it will be seen that in some of Dr. Woodward's figures there is also none. It is not probable that there was any spine in the genus, none being indicated in any of the figures or descriptions published.

Length of body, 14^{mm}; breadth, 14^{mm}; width of the flattened rim or margin, 1^{mm}. Locality, Mazon Creek. No. 218*a*, *b*. Collection of Mr. Lacoe.

Judging by our specimens and Dr. Woodward's figures, Cyclus if restored would have an orbicular body, more or less disk-like or hemispherical, with a cephalic region composed of six segments, which are not, however, indicated externally; this region had a thin margin, as in Prestwichia and Limulus. A pair of median ocelli were probably present, but no compound lateral eyes have yet been discovered. An abdominal region was slightly differentiated, and it was composed of three segments, the third representing that of the embryo Limulus, which in that form eventually becomes the caudal spine. The Cyclus was provided with six pairs of cephalic appendages, which were short, not reaching beyond the edge of the body. With these the animal could creep over the bottom of the shallow, muddy portions of the carboniferous sea. It is not improbable that there were two pairs of abdominal lamellated legs, adapted for respiration, short and broad, and not unlike those of the embryo Limulus. In fact, our conception of the form of the living Cyclus is that it was not much unlike the advanced embryo of Limulus, either in the stage represented in Figs. 17 and 17*a* or 18, 18*a*, and perhaps 19 and 19*a*, of our memoir* of one of which (19*a*) Fig. 8 is a copy. At this stage of development the body of Limulus is hemispherical; seen from beneath the outline of the body is nearly orbicular, the abdominal region completing the circle. If Limulus were arrested at the stage of development when only three abdominal segments had appeared, and the development of the feet and claws had been accelerated and then hatched, it would be, so to speak, a Cyclus.

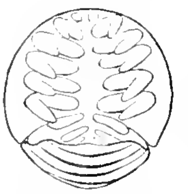


FIG. 8.—Embryo of Limulus, Cyclus stage.

In our first memoir on the development of Limulus we adopted Dr. Woodward's view that Cyclus was a Xiphosuran. In 1868 Dr. Woodward stated: "We must differ from M. de Konink in referring this form to the Trilobites. If truly an adult, it must be placed near to Apus, with the other shield-bearing Phyllopods: if a larval form, it may have been the early stage of Prestwichia, or some other Limuloid of the coal-measures. Nor do we think it in the least probable that the shield of *Cyclus radialis* was flexible or contractile, its original segments being completely soldered together into one piece"; and in 1870 he adds that, from the recent inves-

* The Development of Limulus polyphemus, 1872, Pl. iv. Memoirs Bost. Soc. Nat. Hist., Vol. 1.

* Since this article was sent to the printer, I have received, through the kindness of the author, Mr. B. N. Peach's "Further Researches among the Crustacea and Arachnida of the Carboniferous Rocks of the Scottish Border. Trans. Roy. Soc. Edinburgh, 1882." In this memoir Mr. Peach figures and briefly describes the limbs of Cyclus. "From the fact," he says, "that several of the Survey specimens exhibit limbs, the radiating lines of the sternum are most probably the divisions between the coxæ." Under *Cyclus testudo* Peach, he describes six triangular plates on each side, divided from each other by deep sulci, and converging upon an oral sternum. He also refers to "the jointed cylindrical limbs, the tips of which have not been observed."

tigations of Dr. Lockwood and myself, "these forms may indeed be the larval stages of Prestwichia, Belinurus, etc., the antetypes in Carboniferous times of the modern king crab." "Were it not for the large size of these fossils, some (*C. Harknessi*) measuring five lines in length, three and one-half lines in breadth, and three lines in height, we should be disposed to agree with Mr. Woodward; but, from what is known of the size and form of the freshly-hatched larvæ of Limulus and the Trilobites, I should infer that they were either the larvæ of some unknown genus of Limulidæ, or adult but embryonic forms. The larvæ of Belinurus and its allies, Prestwichia and Euproöps, were, in all probability, closely allied in their form and size at the time of hatching to the larva of Limulus. But on comparing the deep hemispherical form of Cyclus, with the surface of the body deeply lobed over a more or less extent, with the embryo of Limulus before it is hatched (Pl. iv, figs. 18, 18*a*), we find a striking similarity; indeed, we seem to be dealing with a distinct embryonic type of Limulidæ. In Cyclus we have, in a late larval or possibly adult condition, that state of Limulus in which the body is deeply hemispherical, and the abdomen has just been differentiated from the rest of the body, while the deep transverse lobes of the yolk are not yet absorbed, as seen in Pl. iv, figs. 18, 18*a*, in the embryo of Limulus; the cardiac or median lobe being as distinctly marked in Cyclus as in the embryo of Limulus." (Development of Limulus, 1872, p. 189.)

After again reviewing the characters of Cyclus, with the specimen of *C. americanus* before us, we feel confirmed in the views above presented, and would regard Cyclus as the representative of a family of Xiphosura, being an adult form, and embryotypic, to coin a word, of a Limulus, while the Belinuridæ represent the larval Limulus.

Family DIPELTIDÆ Packard.

DIPELTIS DIPLODISCUS Packard. Pl. V, figs. 2, 2a.

Dipeltis diplodiscus Pack., Amer. Naturalist, xix, 293, March, 1885.

This name was proposed for a singular form which is not satisfactorily preserved, so that its exact relations are not readily determinable. The body is suborbicular, flattened, disk-like, sloping regularly and gradually from the median area to the edge; it is divided into two portions; the larger one to be regarded as anterior or the cephalic shield, and the other as posterior, constituting the abdomen (urosome). The edge of the body is very slightly marginate, not broadly so as in Cyclus; nor is the body distinctly trilobate, as in the Belinuridæ and Limulidæ, though unfortunately the median area of the cephalic shield is wanting. The integument is rather thin, showing no traces of segments; its surface may have had a few scattered small tubercles, at least there are slight indications of them. The surface is smooth and shining.

The cephalic shield is nearly twice as broad as long; the posterior lateral angle is well-rounded, with no sign of a lateral spine; in front the edge was probably obtusely rounded; the surface is slightly convex, the disk being low and flat, with no traces of a glabella; the hind edge of the shield is moderately concave, the limits between it and the urosome being clearly indicated by a slight, but distinct, regular, curvilinear suture.

The urosome is about three-fourths as long as, but equal in width to the cephalic shield. The front edge is somewhat arcuate, so that the projecting anterior-lateral angle is directed a little forward, and is quite free from the lateral angle of the cephalic shield, which turns away anteriorly from it, leaving a triangular space between the sides of the two regions. Posterior edge of the urosome regularly rounded, and with a slight margin. No traces of a caudal lobe or spine. Total length, 20^{mm}; total breadth, 20^{mm}; length of cephalic shield, 11^{mm}; breadth, 20^{mm}; length of urosome, 9^{mm}; breadth, 19.5^{mm}. Collection of R. D. Lacoe, 2017 a, b, c, in a nodule from Mazon creek, Morris, Illinois.

This remarkable animal was disk-like in shape, composed of two regions, the head and abdomen or urosome, which are more distinctly separated than in the Cyclidæ; while there are no positive characters to separate it from this group, we would, for the present at least, refer it to an allied family, as it is orbicular, tailless, and consists of a broad, large cephalic shield, with a shorter, distinct, non-segmented urosome.

Family BELINURIDÆ Packard.

PRESTWICHIA DANÆ)Meek) Pl. V, figs. 3, 3a; VI, 1, 1a, 2, 2a.

Bellinurus danæ, Meek and Worthen, Proc Acad. Nat. Sc., Phil., March 1865, Rt. Geol. Surv., Ill. ii, 395, 1866.
Prestwichia danæ Meek, Amer. Journ. Sc., 2d ser., xliii, 257, 1867.
Euproöps danæ, Meek, Amer. Journ. Sc., xliii, 394, 1867.
Meek and Worthen, Rt. Geol. Surv. Ill., iii, 547, 1868.
Packard, Amer. Naturalist, March, 1885.

Head and abdomen (urosome), in the largest specimens, of the same length; in younger specimens the head is rather shorter than the abdomen; head about one-third as long as broad; genal spine about two-thirds as long as the head, and turning at nearly a right angle with the straight hinder edge of the cephalic shield; the spine as a whole is directed somewhat outward, nearly reaching a point about opposite the hinder edge of the third abdominal segment. Median lobe of the head or glabella, rather deeply excavated in front; at the bottom of the excavation are situated traces of the simple eyes, which have the same situation and shape as in Limulus. The small compound eyes are situated on the outer anterior angle made by the sides of the glabella and are of nearly the same relative size and in the same general situation as in the larval Limulus, though placed a little nearer the front margin. The eyes themselves are small, oval and prominent. The sides of the glabella are produced behind into a sharp spine, projecting backwards over the base of the abdomen.

The abdomen (or urosome) is from one-fourth to one-third broader than long, and is composed of eight distinct segments, including the caudal spine; the body of the abdomen is full, convex, and distinctly trilobate, the median or cardiac lobe being in general about a third narrower than the lateral lobes or plenra, and contracting in width towards the fifth segment. The sutures between the segments on the lateral lobes are very distinct, being raised, narrow ridges, prolonged into and forming the hinder edge of the long, sharp, slightly curved, lateral spines; of these lateral spines those on the first and second segments are the narrowest and most acute, that on the seventh the widest and most obtuse. In the cardiac lobe the third abdominal segment bears a high rounded tubercle, and there is one about twice as large on the sixth segment; those on the other segments are small, and in most of the specimens there are traces only of those on the third and sixth segments. The caudal spine (representing the eighth abdominal segment) is somewhat enlarged at the base; it is three-cornered in section, much as in Limulus, the surface is smooth, and it is about three-fourths as long as the abdomen.

Length of entire body (largest specimens), 60^{mm}; breadth, 53^{mm}.
Length of cephalic shield, 24^{mm}; breadth, 53^{mm}.
Length of lateral cephalic spine, 15^{mm}; breadth, near base, 3.5^{mm}.
Length of abdomen (urosome) (not including the caudal spine), 23^{mm}; breadth 35^{mm}.
Length of longest lateral abdominal spine, 6^{mm}.
Length of caudal spine (telson), 15^{mm}.

The smallest specimen is 10^{mm} in length, and 12^{mm} in width, the caudal spine being less than one-half as long as the abdomen.

Description of the cephalic appendages.

In a nodule from Mazon Creek received from Mr. J. C. Carr, containing the remains of a specimen 55^{mm} across the shield (Pl. VI, figs. 2, 2a), the cephalic appendages are more or less distinctly preserved. Of the first pair there are faint traces, the two small limbs lying parallel to each other and in the same position as in the larval Limulus, and of nearly the same proportions. The impressions of the succeeding limbs are distinct; the second third, fourth, and fifth pairs are of the same size, the fifth pair being perhaps a little longer, as the tips extend near the edge of the cephalic shield. All four pairs, *i. e.*, second to fifth, are chelate, the forceps being well developed and plainly visible in the third and fourth pairs, as these limbs are turned on their side; the fifth pair are undoubtedly chelate, but lie so that the outline is a simple point. The sixth pair differs

from the others in ending abruptly, the penultimate joint being long and of the same width throughout, and truncate at the distal end, where it gives rise to three small, sharp spines; there are also traces of a terminal minute joint from which two spines arise.

Length of second, third, fourth, and fifth pairs of legs, 16^{mm}.
Length of sixth pair, 17^{mm}.
Length of penultimate joint, 6^{mm}.
Thickness, 1^{mm}.

The legs are nearly identical in shape and length with those of the larva of Limulus described and figured in my Development of Limulus (Pl. I, figs. 24^{a}, 25^{a}, and 23^{d}), though perhaps a little shorter, as they do not reach beyond the edge of the cephalic shield. It thus appears that in respect to its limbs as well as the shape and proportions of the body the Prestwichia resembles the larval Limulus. Thus Limulus in its development passes through a trilobitic, and afterwards a Belinurid stage.

PRESTWICHIA LONGISPINA Packard. Pl. V, fig. 4.

Euproöps longispina Pack., Amer. Naturalist, xix, 292. March, 1885.

The specimen upon which this species is founded is Mr. Lacoe's Nos. 215$^{a, b}$ (impression and reverse), and was probably a molted skin (Pl. V, fig. 4). The body is considerably distorted by pressure, but the specific distinctness from *P. danæ* is marked. The species will be readily distinguished by the very long genal spines; they extend nearly or quite to a point opposite the base of the caudal spine. The abdomen appears to be narrower in proportion to the cephalic shield than in *P. danæ* while the genal spines are longer and narrower. The caudal spine is not well preserved.

Length of body (not including the caudal spine), 20^{mm}.
Length of head, 10^{mm}.
Length of abdomen, 10^{mm}.
Breadth of cephalic shield, 24^{mm}.
Breadth of abdomen, 13^{mm}.
Length of lateral cephalic spine, 13^{mm}.

Pittston, Pa., Butler mine, Nos. 215$^{a, b}$, collection of Mr. Lacoe.

In another larger specimen (Lacoe's No. 214^{a}, Pl. VI, fig. 3), the glabella, with the eyes, ocelli, and a part of the left lateral spine are preserved. Whether this is the same species as *P. longispina* I cannot tell with certainty, as the genal spines are not sufficiently well preserved, but provisionally it may be regarded as belonging to the species under consideration. The median lobe of the head is larger in proportion to the entire cephalic shield than in *P. danæ*, and the eyes are nearer the lateral margin. The ocelli are situated on the median ridge of the lobe, somewhat behind the indentation between the lobes. The individual is without doubt a Prestwichia having the same number of abdominal segments as in *P. danæ*.

Length of body (without the caudal spine), 30^{mm}.
Breadth of cephalic shield (estimated), 37^{mm}.
Length of cephalic shield, 17–18^{mm}.
Length of abdomen, 13^{mm}.
Breadth, 23^{mm}.
Estimated length of lateral cephalic spine, 15^{mm}.
Distance between the compound eyes, 17^{mm}.
Distance from ocelli to the front edge of body, 6^{mm}.
Distance from ocelli to hinder edge, 21^{mm}.

Oakwood Colliery, Wilkes Barre, Penn., collection of Mr. Lacoe, No. 214^{a}.

Regarding the position of the Illinois and Pennsylvania beds containing these fossils, Mr. Lacoe writes me: "The horizon of the Pennsylvania specimens of Euproöps is much higher than that of Mazon Creek. The latter is at the very base of the productive coal-measures in shale over the bottom seam of coal. The specimen from the Butler mine, Pittston, is from shale over coal 'E' (Mammoth vein), at the top of the lower productive coal-measures, about 300 feet above, and that from the Oakwood colliery is either from the same horizon or the bottom of the lower barren

measure next overlying it. The shaft from which it was taken, penetrating both, the exact position of the rock containing it could not be ascertained when we discovered it in the 'dump' or rock pile." Another specimen from Scotch Hill railroad cut, Pittston, Pa. Coal E. Lacoe's No. *C.* $3\frac{3}{1}$–34.

Note on the validity of the Genus Euproöps.

By referring to the synonymy of *Prestwichia danæ*, it will be seen that in 1865 it was referred by Messrs. Meek and Worthen to Belinurus for reasons given in Palæontology, vol. iii, of the Geological Survey of Illinois, p. 547. After the appearance of Dr. H. Woodward's paper read before the Geological Society of London in 1866* in which the genus *Prestwichia* was separated from Belinurus, the American form was referred to the new genus, *Prestwichia*, by Mr. Meek.

"At a later date (February, 1867), Mr Woodward published excellent figures in the Quart. Jour. Geol. Soc., London, vol. xxiii, pl. 1, of the typical forms of both *Preswtichia* and *Belinurus*. From these it became evident that the peculiarities of the ridges of the head of the form on which he founded the genus *Prestwichia*, and which we had supposed probably due to some accident, really exist. Consequently, our type was regarded as being generically distinct, and the name *Euproöps* was proposed by one of us for it. Mr. Woodward, however, has since expressed the opinion that these differences are probably of scarcely more than specific value. (See Geol. Mag., Jan. 1868, vol. v., p. 2.) Without professing to have made an especial study of the fossil *Crustacea*, on which Mr. Woodward is well known to be an eminently reliable authority, we would state that we can scarcely doubt that a comparison of *specimens* would lead him to the conclusion that the American form is at least subgenerically, if not generically, distinct from Prestwichia."

Finally the authors state that Euproöps differs from Prestwichia "not only in the position of the eyes, and the form and size of the glabella, or central area of the cephalothorax, but in the entire arrangement of the ridges and included areas of the same." Fig. 9. is from an electrotype of a cut published by Messrs. Meek & Worthen in illustration of their genus Euproöps.

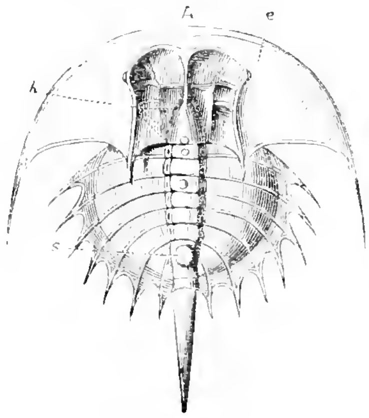


FIG. 9.—*Euproöps danæ*. M. & W After Meek

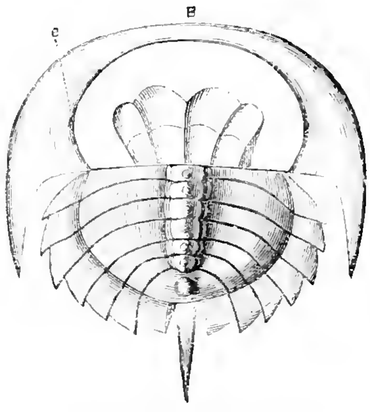


FIG. 10.—*Prestwichia rotundatus*. After Woodward.

After repeated examinations of the series of about a dozen specimens from the collections of Messrs. Lacoe and Carr, I am at a loss to find valid characters for the genus Euproöps. In one example of *P. danæ*, the glabella or middle lobe of the head, is distinctly divided into four sublobes, as in Woodward's figure of *P. rotundatus*; again the lack of lateral abdominal spines in his figure of *P. rotundatus* appears to me to be due to the imperfect state of preservation of the specimen, as some of the Illinois specimens do not show them; again the spines projecting from the sides of the glabella over the base of the abdomen, and represented as wanting in Woodward's figures, are wanting in certain Illinois specimens. As to the position of the compound eyes in *P. rotundatus* as represented in Woodward's figure, I am inclined to believe that the author and artist have been in error. I should not venture to make such a statement if in our Illinois and Pennsylvania specimens of Prestwichia and Belinurus the position of the eye were not invariably on the outer

*On some points in the structure of the Xiphosura, etc., Quart. Journ. Geol. Soc, Feb. 1867.

angle of the glabella, in a position homologous with their situation in Limulus. I venture then to give the opinion that the apparent differences between Prestwichia and Euproöps, as stated by Messrs. Meek and Worthen, did not exist in nature, and that the genus Prestwichia was common to both Europe and North America during the Carboniferous Period. It is interesting in this connection to observe that the descendants of the Belinuridæ in Europe, survive in the Solenhofen Limuli until the Jurassic, and disappear during the Cretaceous period, not to arise again on the western coasts of the old world, while in North America, so far as the record shows, the type became extinct during the Mesozoic and Tertiary, to reappear in the Quaternary and present period.

As regards the differences between Belinurus and Prestwichia, the former genus is the higher form, approximating Limulus in the consolidation of the eighth and ninth abdominal segments (forming the "abdomen" so regarded by Dr. Woodward) and in the very long caudal spine. In Prestwichia there is one abdominal segment less than in Belinurus, the short caudal spine forming the eighth.

BELINURUS LACOEÏ Packard. Pl. V, fig. 5.

Belinurus lacoei Pack., Amer. Naturalist, xix, 292, March, 1885.

Cephalic shield of the usual shape and length in proportion to the abdomen; the front margin as usual; the genal spine long, acute, extending obliquely outwards to a point parallel with one either a little behind the middle of the abdomen, or, in the older, larger specimens, nearly to a point parallel with the base of the caudal spine. The median lobe is, as usual, divided by the median line into two sublobes, so that the front edge of the entire lobe is indented in the middle; each sublobe contracts in width posteriorly behind the ocular or lateral angle bearing the compound eyes. The ocelli are not visible, but the compound eyes are partly preserved; they are small, and of the usual kidney shape. The abdomen is much more rounded than in the European *B. reginæ*, being twice as broad as long. It consists (including the caudal spine) of nine segments. The median lobe is as broad at the end as at the base next to the thorax; there is a median tubercle on each segment, those on the third and last segment being larger than the others. The margin of the abdomen is broad and thin, giving rise to broad, acute, lateral spines. The caudal spine is very long and slender, a little swollen at the base; it is also triquetal, as in Limulus; it is nearly one-half longer than the body, *i. e.*, longer than the whole body by the length of the head, and ending in a fine, slender, needle-like point.

Length of the best preserved specimen 33^{mm} (including the caudal spine).
Length of body, 15^{mm}.
Length of caudal spine, 18^{mm}.
Length of cephalic shield, 7^{mm}; breadth at base of lateral spine, 16^{mm}.
Length of lateral spine, 4–5^{mm}.
Length of abdomen, 8^{mm}; breadth (not including the spines), 12^{mm}.

In nodules at Mazon Creek, Illinois; Nos. 210^{hi}, 210^{ij}, 210^{wx}, $212^{a.b}$; 213^{a}, collection of Mr. Lacoe.

While having the same number of abdominal segments, this species, the first representative of the genus which has occurred in America, differs from *B. reginæ* chiefly in the more rounded, less triangular outline of the abdomen, and in the smaller lateral abdominal spines. It is probable that in Dr. Woodward's figure of *B. reginæ* the compound eyes are not correctly placed. In our specimens of Belinurus they have the same relative situation as in *Prestwichia danæ* and *longispina*.

SYNOPSIS OF THE NORTH AMERICAN XIPHOSURA.

Suborder XIPHOSURA.

Family 1. CYCLIDÆ Pack.

Body disk-like, orbicular; abdomen composed of three segments, scarcely if at all differentiated from the cephalic shield; cephalic limbs nearly as in the larval Limulus; size small.

Genus *Cyclus* De Koninck, with the characters of the family.

Cyclus americanus Pack.

Family 2. DIPELTIDÆ Pack.

Body disk-like, elliptical; abdomen differentiated from the cephalic shield, smooth, no segments indicated.

Genus *Dipeltis* Packard, with the characters of the family.

Dipeltis diplodiscus Pack.

Family 3. BELINURIDÆ Pack.

Body limuloid in general shape; cephalic limbs as in the larval Limulus; shield with long slender genal spines; abdomen with the segments distinct; caudal spine short or long.

Genus *Prestwichia* Woodward. Eight abdominal segments, including the short caudal spine.

Prestwichia danæ Meek.

Prestwichia longispina Pack.

Genus *Belinurus* König. Nine abdominal segments, including the very long, slender caudal spine; segments 7 and 8 consolidated.

Belinurus lacoeï Pack.

Family 4. LIMULIDÆ Zittel.

Body longer than broad; abdomen with segments consolidated; six pairs of abdominal limbs, five pairs having over a hundred pairs of gill-leaves.

Genus *Protolimulus* Packard.* Seven abdominal segments, including the large thick caudal spine.

Protolimulus eriensis (Williams).

Genus *Limulus* Müller. Cephalic limbs large; body longer than broad; abdomen with 9 segments; caudal spine longer than the body.

Limulus polyphemus (Linn.).†

* In a notice of a new Limuloid Crustacean from the Devonian, Amer. Journ. Sc., July, 1885, p. 45, Prof. H. S. Williams described an interesting Limuloid from the Devonian of Erie County, Pennsylvania (associated with typical Chemung fossils). It is described as *Prestwichia eriensis*, the author remarking that "its identification with Prestwichia must be regarded as provisional." He then adds: "The following characters exhibited in the specimen are regarded as generic and as locating it with genus Prestwichia of Woodward: (1) the elliptical head shield; (2) the genal spines which proceed backwards more directly than in any described species of the genus; (3) the thoracico-abdominal segments anchylosed to form a buckler, to which is attached (4) a long telson. The general outline of the whole animal resembles that of the modern Limulus." We have ventured, without having seen the specimen, to regard this form as probably a member of the family Limulidæ, and the forerunner of Limulus. It is certainly not a Prestwichia. The body is apparently longer than broad, and in outline it leaves a strong resemblance to the young Limulus after its first moult. This is seen in the shape of the abdomen and the caudal spine and in their relations to the rest of the body. It also seems probable that the abdominal segments were not free; in this respect it differs from the Belinuridæ, especially Prestwichia. Judging by the number of lateral spines, the abdomen was composed of 6 segments exclusive of the caudal spine, thus differing from Prestwichia, which has 7, also from Limulus, which has 8 pairs of lateral spines. We therefore venture to give it the generic name of Protolimulus, and to regard it as standing at the base of the family to which Limulus belongs. Its occurrence in the Devonian makes it a connecting link between the Upper Silurian Neolimulus and the Carboniferous and Jurassic Limuloids. We are indebted to Prof. Williams for the use of figures illustrating his *P. eriensis*.

† Besides the American species, there are three others living, viz, *L. moluccanus*; (East India) *L. longispina* Van der Hoven, Japan; *L. rotundicauda* Latr., Molucca Is. and Malacca.

FIG. 11.—*Protolimulus eriensis* (Williams). A sandstone cast of the under surface, natural size.

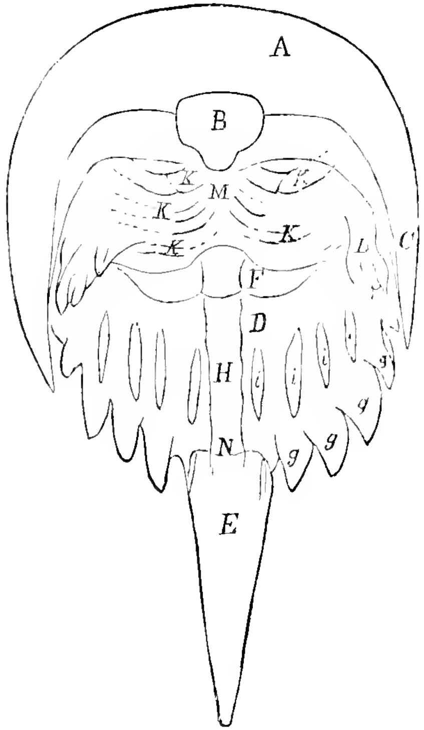


FIG. 12.—Diagram of Fig. 11. A, cephalic shield; B, ?hypostoma; C genal spines; D, thoracico-abdominal buckler; E, telson; *ii*, longitudinal ridges of the buckler; K K, portions of the gnathopodes; L, ?foliaceous terminations of the last gnathopodes; M, position of the mouth.

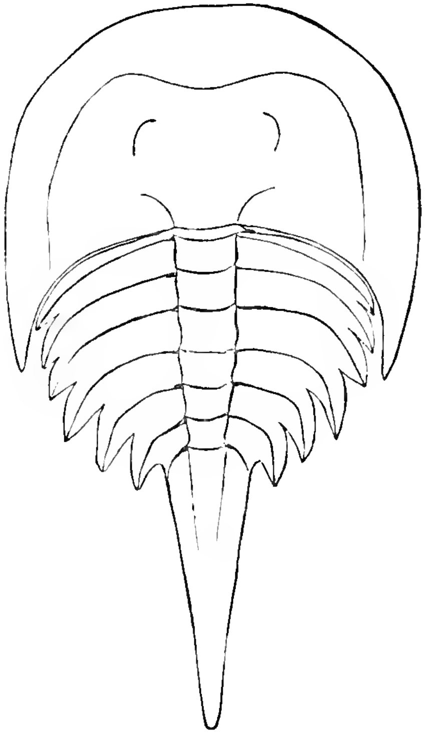


FIG. 13.—Theoretical diagram of the upper side.

PROTOLIMULUS ERIENSIS. After Williams.

The individual development of Limulus an epitome of that of the Xiphosura.

It is interesting to observe a clearly marked exemplification of the parallelism between the embryonic or ontogenetic development of Limulus and the geological succession as well as evolution of the suborder of which it is a type. We have already compared the orbiculo-hemispherical form of Cyclus with that of Limulus in the early stages of its embryonic life. The parallelism is striking. Cyclus may therefore be called an embryonic form. Again, in Prestwichia there is a close resemblance to Limulus shortly before it leaves the egg, in what we have called the trilobitic stage, a stage antecedent to the true larval stage, in which the abdominal segments become consolidated. Prestwichia may then be properly designated as a larval form, while Cyclus was an embryonic form. The latter genus embraces eleven species (ten in Europe), which exist in beds containing the species of Belinuridæ. One cannot regard it as a retrograde form however, but as an embryonic Xiphosuran, whose development became accelerated, adapting it for active adult life. While the specimens of Cyclus have not yet shown the presence of compound lateral eyes, it is not impossible that the animal was provided with a pair of median simple eyes. This indicates that these were the primitive visual organs, and that the compound lateral eyes of the Belinuridæ and Limulidæ were secondary acquisitions, and that their simple eyes are legacies left by their Cyclus-like ancestors.

Cyclus, and perhaps Dipeltis, appear to represent Agnostus among Trilobites, and the similarity between all these simple types indicates a community of descent.

The Suborder SYNZIPHOSURA.

In the Upper Silurian beds of Europe have been revealed a number of exceedingly interesting forms, which appear to be Merostomata, but not true Xiphosura. They serve, on the one hand, to connect the Xiphosura with the Eurypterida, and also strongly suggest the community of origin of the Merostomata and Trilobita. They have been associated by Dr. Woodward with the Eurypterida,* but it seems to us, in the light of our present knowledge of the latter suborder and of the Xiphosura, that they are types of a third group or suborder. Perhaps the more aberrant form is Bunodes of Eichwald. All the genera have a caudal spine or telson. They are, besides *Bunodes*, *Hemiaspis* Woodward, *Pseudoniscus* Nieszkowski, *Exapinurus* Nieszk., and perhaps *Neolimulus* Woodward belongs with them, though the last form connects the Xiphosura and Synziphosura. They possess nearly as high an antiquity as the Eurypterida, but did not persist so long, as none have been discovered in the Devonian or Carboniferous rocks; hence we would infer that they were the forerunners of the Xiphosura rather than actual members of the group. In a word, the merostomatous ordinal tree divided into three main branches—*i. e.*, the Eurypterida; the forms under consideration, which may be designated as the Synziphosura; and the genuine Xiphosura. In the Synziphosura the head forms a solid plate, with a slightly marked glabella or median lobe. Compound eyes are present in Pseudoniscus, and in Exapinurus the head is produced laterally into large genal spines. All have free uromeres or abdominal segments, and in all except Bunodes, in which the pleurum is shaped and marked as in Trilobites, the uromeres possess lateral projections or spines. None of them show traces of limbs or of simple eyes, and all are of moderate size.

The Synziphosura may be divided into three families, which may be diagnosed as follows (these groups appear to be, on the whole, equivalent in rank to the families of Trilobites):

1. Head rounded; no genal spine; abdomen divided into a "thorax," consisting of six trilobite-like segments, with diagonal pleural lines; "abdomen" of four segments, besides the large telson (Bunodes and Exapinurus). *Bunodidæ* Packard.

2. Head one-half broad as long, with several genal spines; abdomen triangular, with nine segments and a short telson (Hemiaspis). *Hemiaspidæ* Zittel (restricted).

3. Body oval; head short; large compound eyes; nine abdominal segments besides a short telson (Pseudoniscus). *Pseudoniscidæ* Packard.

4. Head-shield short and broad; abdomen very broad, of nine segments besides the telson (Neolimulus). *Neolimulidæ* Packard.

* Quart. Journ. Geol. Soc., Feb., 1867.

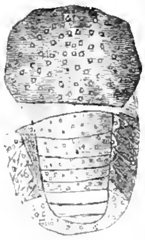


Fig. 14.— nodes. After Nieszkowski.

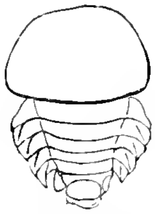


Fig. 15.—Bunodes After F. Schmidt

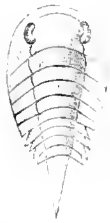


Fig. 16.—Pseudoniscus. After Nieszkowski.

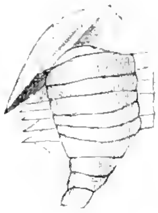


Fig. 17.—Exapinurus. After Nieszkowski.

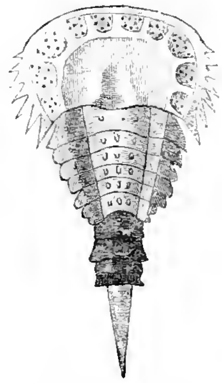


Fig. 18.—Hemiaspis. After Woodward.

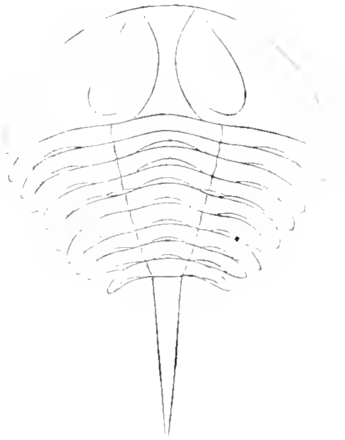


Fig. 19.—Neolimulus. After Woodward.

After the foregoing classification was mostly written out, we found that Professor Zittel, in his excellent Handbuch der Palæontologie, Bd. i, 640, 1885, has divided the suborder of Xiphosura into two families:

1. *Hemiaspidæ*, with the following genera: Bunodes (Exapinurus Nieszk.) subgenus Hemiaspis, Pseudoniscus, Neolimulus, Belinurus, and Prestwichia; while Cyclus and ? Halycine are regarded as genera of uncertain position.
2. *Limulidæ*, Limulus.

It seems to us that this is scarcely a natural classification, and that it would be better to separate the Silurian forms mentioned above from the genuine Xiphosura, especially as we know nothing of the nature of their appendages, and to assign them, at least provisionally, to a group distinct from the genuine Xiphosura, especially since we now know something definite as to the nature of the cephalic appendages of Cyclus and Prestwichia, their resemblance to those of the existing Limuli being remarkably close. Certainly Bunodes, in which there are, according to F. Schmidt's late researches,* as stated and figured by Zittel, besides a four-jointed abdomen, a "thorax" composed of "six trilobite-like, movable segments," cannot well be allowed a position in the genuine Xiphosura. Moreover, the pleura of the single segments show a diagonal longitudinal ridge. This mark is a peculiarity of the pleura of some trilobites, and does not occur in any genuine Xiphosura, and aids in lending to Bunodes a trilobitic appearance.

If we separate Bunodes from the true Xiphosura, Hemiaspis will have to go with it, since it has a rounded cephalic shield, shaped somewhat as in Bunodes, but broader. We should not, with Zittel, regard it as a subgenus of Bunodes, because the "thoracic" segments have on the free sides no diagonal ridge, and the cephalic shield is ornamented with large spines, which perhaps indicate the head segments of the embryo. In both genera no eyes have yet been discovered. For the present we should, on the whole, regard the two genera as representing different families.

* F. Schmidt, Miscellanea Silurica III. Die Crustaceen fauna der Eurypterusschichten von Rootziküll auf Oesel. Mém. de l'Acad. impér. de St. Péterbourg, 7e ser., xxxi, 1883.

Johannes Nieszkowski, Zusätze zur Monographie der Trilobiten der Ostserprovinzen nebst der Beschreibung einiger neuen obersilurischen Crustaceen. Dorpat, 1859.

In Pseudoniscus we have another form which suggests a relationship to the Trilobites. Our figure is copied from Woodward's restoration. Nieszkonski, the original describer, remarked, "On the inner side of the shield we notice a place cut out, with the convexity looking outward, which should certainly be regarded as the outer edge of the eye."

The foregoing remarks are suggested by a study of the figures and descriptions of these remarkable forms, and as they are not based on a study of the specimens themselves, they will be taken only for what they are worth. But the fact remains that we have, side by side with the Eurypteridæ in the upper Silurian strata, a group which does not apparently belong to either the Eurypterida or genuine Xiphosura of the Carboniferous and later periods, and to which it seems best to assign, temporarily at least, an intermediate position. The group also is of great interest as serving to bridge over the gap between the Merostomata and Trilobita.

The following view will express the relations of the three suborders:

Order MEROSTOMATA.

1. *Eurypterida.* 2. *Synziphosura.* 3. *Xiphosura.*

HISTORICAL REVIEW.

I.—*History of the Xiphosura.*

In 1764 Gronovius, in the second fasciculus of his Zoophylacium Gronovianum, p. 220 (according to Van der Hoeven, for we have not seen this work), proposed the name *Xiphosura.* His work appeared in three fasciculi, bearing date 1763 to 1781, the second fasciculus dated 1764.

The name Limulus was first proposed by O. F. Müller (Entomostraca, 1785, p. 124), and adopted by Fabricius (Ent. Syst., 487, 1893).

The name *Limulus polyphemus* (Linn.) was bestowed by Latreille in his Histoire Naturelle des Crustacés et des Insectes, tom. 4, p. 96, 1802.

In 1798 Latreille, in Cuvier's Tableau élémentaire de l'Histoire Naturelle des Animaux, placed the Limuli in the Crustacea, under the Monoculi.

Previous to 1806, the exact year we have not been able to ascertain, Latreille (Suite à Buffon, Sonnini, Paris, 1798–1807) assigned Limulus to the Entomostracan order 1 Xiphosura (fide Milne Edwards).

In 1806 Latreille (Genera Crustaceorum et Insectorum, i, 10) placed Limulus in order 1 Xiphosura of Legio 1 Entomostraca.

In the same year Duméril (Zool. Anal.) associated Limulus with Caligus, etc.

In 1809 W. Martin "gave a figure and short description of a Limulus crustacean from the coal measures, which he included with the Trilobita."

In 1810 Latreille (Considérations générales, etc.) assigned Limulus a place under the Entomostraca in Family 1, Clypeaces, Aspidiota, associating it with Apus, Caligus, and Binoculus. The term *Xiphosura* does not appear.

In 1835 Latreille (Familles naturelles du Règne Animal) places the Xiphosura between the Phyllopods, the Trilobites, and the Siphonostoma.

In 1828 Straus Durckheim (Considérations générales sur l'anatomie comparée des Animaux articulés) referred Limulus to a new order, *Gnathopoda*, forming the eighth order of Crustacea, which he placed between the Decapoda and Arachnida.

After the publication of his "Considerations," Straus-Durckheim removed the Gnathopoda from the Crustacea to the Arachnida, as will be seen by the following extract from Lankester's "Limulus an Arachnid" (Quart. Journ. Micr. Sc., 506, 1881):

> Straus Durckheim maintained that Limulus should be classified with the Arachnida, but the publication of his views on the subject appears never to have taken a very definite or satisfactory form. In fact, the only record of Straus Durckheim's teaching on this subject which I can find is in the French translation of Meckel's "General Treatise on Comparative Anatomy." MM. Reister and Alph. Sanson carried out this translation and added many notes in the form of appendices to each volume. At the end (p. 497) of the sixth volume, which bears the date 1829–1830, there is a note headed "Sur l'appareil locomoteur passif des Arachnides," which appears to be an abstract of a memoir "On the

Comparative Anatomy of the Arachnida," read to the Academy of Sciences June 1, 1829, but never, I believe, published. M. Straus Durckheim communicated its contents to MM. Reister and Sanson. From this note I submit a few extracts. The authors commence:

"La classe des Arachnides, dans laquelle M. Straus comprend le genre Limule, formant à lui-seul un ordre designé sous le nom de *Gnathopodes* et dont il isole les *Pycnogonides* qu'il renvoie aux Crustacées, offre dans la disposition de son squelette et des muscles qui en meurent les diverées pièces, des particularités tellement tranchées qu'on ne peut, y méconnaître un type différent. C'est de ce squelette que sont tirés les traits principaux propres à caracteriser la classe des Arachnides en général, et qui consiste dans la *disposition des pattes rayonnant sur un sternum commun, dans la présence d'un sternum cartilagineux intérieur, dans l'absence d'antennes.*"

The Arachnida are then divided into three orders, "les pulmonaires, les branchifères, et les trachéens," but it is not explained whether the term "gnathopodes" is to be regarded as simply a synonym of the order "branchifères."

With regard to the internal sternum, the citation of the views of M. Straus runs as follows:

"Dans l'intérieur du thorax de tous les *Arachnides*, à l'exception peut-être des *Acarides* dont la plupart des espèces sont trop petites pour qu'on puisse les dissequer et connaître leur organisation, on trouve une pièce cartilagineuse diversement configurée suivant les familles, et placée dans le thorax au dessus du *sternum*, cette pièce, à laquelle convient le nom de sternum intérieur, est maintenue librement par le moyen de plusiers muscles qui ce conduit de différents points de sa surface sur le bouclier, ou sur le sternum extérieur auquel ils se fixent. Elle sert en outre de point d'insertion à un certain nombre de muscles des pattes."

In Cuvier's Règne Animal, nouv. édit., 1829 (tom. iv), the group named by Latreille, *Pœcilopoda*, is characterized and described as the second order of Entomostraca. The order consists of two families: Xiphosura (genus Limulus) and Siphonostoma (Caligus, Argulus, etc.). As the group Pœcilopoda, by its founder, includes the parasitic Copepoda besides Limulus, it seems advisable to drop it, retaining the term Xiphosura, which has never been applied to any other animal than Limulus and its allies. On p. 46 he remarks: "De cet ordre de crustacés on arrive à la classe des ARACHNIDES, dont l'organisation, en général, approche beaucoup de celle des Limulus."

In 1830 Milne-Edwards (Ann. des Sc. Nat., xx, mars 1830) adopted the order Xiphosura, placing it below the Siphonostomata.

In 1834 Milne-Edwards (Hist. Nat. des Crustacés) retained the order Xiphosura.

Straus-Dürckheim's views were more explicit than supposed by Professor Lankester, as in Straus's work, published in 1842, entitled "Traité pratique et théoretique d'Anatomie comparative," etc., vol. 2, 169, we find the following statement:

J'ai formé l'ordre des *Gnathopodes* avec le seul genre *Limulus*. Ces singuliers animaux ont été rangés parmi les Crustacés par tous les naturalistes qui, ne connaissant pas leur organisation, les plaçaient dans cette classe par cela seul qu'ils ont des branchies, tandis qu'ils s'en distinguent essentiellement par le reste de leur organisation, en offrant les plus grandes analogies avec les Arachnides! et l'existence des branchies ne saurait à elle seule constituer un caractère suffisant pour les éloigner de ces derniers, vu que dans cette classe les organes de la respiration n'ont plus cette grande prépondérance sur les autres appareils du corps, pour les tenir sous leur dépendance, comme cela a lieu chez les vertébrés; ce qui est prouvé par l'analogie qui existe entre les Arachnides pulmonaires et les trachéens, qu'on ne saurait séparer.

Dana (1852) in his Crustacea of the U. S. Exploring Expedition, proposed the order *Merostomata* for Limulus exclusively, which he places in the tribe *Limuloidea*. He makes no mention of the Eurypteridæ. The *Pœcilopoda* in Dana's system forms the first suborder of *Cormostomata*, and include the Ergasiloidea, Caligoidea, and Lernæoidea.

In 1866 Hæckel (Generelle Morphologie der Organismen, ii, lxxxix) regarded the *Trilobita* as forming the third legion of Branchiopoda. They are in his system succeeded by the sixth subclass of Crustacea, the *Pœcilopoda*, which embraces the two legions of *Xiphosura* and *Gigantostraca*. The latter name is proposed for the *Pterygotidæ* and *Eurypteridæ* alone. As Hæckel's Gigantostraca appears to be exactly synonymous with Dana's Merostomata as amended by Woodward, the awkward, meaningless term, which has never been defined, should be discarded. It has, however, been adopted by Dohrn in 1871 (Zur Embryologie und Morphologie des Limulus Polyphemus, Jena. Zeits, vi, 1871), and by Claus, though in a greatly extended, and it seems to us an unwarrantable, sense. Dohrn remarks:

Limulus is nearest related to the Gigantostraca. Both appear to be related to the Trilobites, though this relationship cannot be established in all the details. The morphological and genealogical relations of these three families to the Crustacea are not such as to be surely determined; perhaps they will remain always doubtful. That they are related to the Arachnida we are not, as the matter now stands, in a position to allow. So it only remains for us to put these three groups under a common name, for which I might adopt Hæckel's expression "Gigantostraca," and let them take their place in the system with (*neben*) the Crustacea.

II.—*Affinities of the Eurypterida to the Xiphosura (Limulidæ) and the formation of the order Merostomata as at present received.*

In 1825 Dr. J. E. De Kay described and figured the first (an American) species of Eurypterus known (*E. remipes*), and referred it to the class Crustacea and to the order Branchiopoda.

In 1844 L. Agassiz remarked of Pterygotus:

I am rather inclined to believe that this singular animal will become the type of a family intermediate between the *Trilobites* and the *Entomostracans* in which perhaps, the *Eurypteri* and the *Eidotheæ* will some day be included.

We have given on pp. 177, 178 of our essay on "The Development of Limulus" (1872) a history of the views of James Hall, Salter, and others, especially the first-named, who proved that the Eurypterida belonged to the same order as Limulus.

In 1866 in his elaborate "Monograph of the British fossil Crustacea, belonging to the order Mesostomata," Dr. H. Woodward formally united the Eurypterida in the same order with Limulus, remarking:

Having long been convinced of the propriety of expressing in some suitable manner the correctness of the conclusions of Professors Agassiz and James Hall as to the close affinity existing between the *Eurypterida* and the *Xiphosura*, and being fully persuaded at the same time that they naturally form two distinct although closely related groups, I have ventured to unite them in the Order MEROSTOMATA—a name proposed by Dr. J. D. Dana for the recent king-crabs only, retaining at the same time the names *Eurypterida* and *Xiphosura* as suborders.

In 1872 we adopted this classification, which seems eminently natural, and has since been adopted by a number of leading zoologists.

In 1868 Claus (Grundzüge der Zoologie) characterized the order *Pœcilopoda*, but in the third edition of this work (1876) the Pœcilopoda (restricted to Limulus), though placed between the fourth order, Phyllopoda, and fifth order, Arthrostaca, in the Crustacea, and at the end of the Phyllopoda, are associated with the Trilobita in a special group to which no special rank is assigned.

III.—*Transfer of the Merostomata (with the Trilobita) to an independent class.*

In 1869 Huxley stated in the "Academy" (November 13):

The Xiphosura have such close morphological relations with the Arachnids, and especially with the oldest known Arachnidan, Scorpio, that I cannot doubt the existence of a genetic connection between the two groups.

In 1871 Prof. E. Van Beneden (Comptes Rendus de la Soc. Ent. Belgique, October 14, 1871; Annals and Mag. Nat. Hist., January, 1872) remarked:

The Limuli are not Crustacea; they have nothing in common with the Phyllopoda, and their embryonic development presents the greatest analogy with that of the scorpions and other Arachnida, from which they cannot be separated. * * * The Trilobites, as well as the Eurypterida and the Pœcilopoda, must be separated from the class Crustacea, and form with the Scorpionida and the other Arachnida a distinct branch, the origin of which has still to be ascertained.

In 1872 A. Milne Edwards (Annales des Sc. Nat.) published his important researches on the internal anatomy of Limulus, which showed that Limulus essentially differs from the Crustacea. In the same year we attempted to show the close affinities of Trilobites to Limulus.

In 1876, according to Claus's own statement (Annals and Mag., July, 1886, p. 56), referring to his change of views as to the position of Limulus, he remarks:

Even in the work entitled "Untersuchungen über die genealogische Grundlage des Crustaceensystems" (Vienna, 1876) I adhered to the views of those who, like Straus-Dürckheim, regard *Limulus* and Branchiate Gigantostraca as allied to the air-breathing Arachnoidea, and the latter as having proceeded from the former, although, having regard to the possibility of a still undemonstrated Nauplius stage, I considered it probable that the common origin of the true Crustacea was rather after than before the Nauplius period of the Stem-Crustacean. In the case of *Limulus* and the Scorpions I also asserted the homology both of the six pairs of limbs of the cephalothorax, and, with reference to the developmental history, of the six pairs of limbs of the præabdomen, of which the second pair represents the comb-like organ of the Scorpions, while the following four pairs immediately undergo retrogression (p. 110). In the "Grundzüge der Zoologie" of the year 1880 I went so much further as to divide the Branchiata, or Crustacea, *sensu latiori*, into *Encrustacea* (with the Entomostraca and Malacostraca) and *Gigantostraca* (with no certain traces of the Nauplius stage), and accordingly I affirmed expressly of the Tracheata *that in opposition to the more ancient Branchiata they "were not referable to a unitary origin, since the Arachnoidea, which are derivable from the Gigantostraca, stand opposite to the Myriapoda and Insecta, which are united by a closer affinity"* (p. 515).

In 1885 and 1886 (Annals and Mag. Nat. Hist., July, 1886) Claus regarded the Gigantostraca as a class intermediate between the Crustacea and Arachnida. He thinks that the Arachnida descended from the Gigantostraca, adding, "I by no means affirm the *Arachnoidal nature of Limulus.*"

In 1879, in our Text-book of Zoology, as the result of Milne Edwards's researches, we divided the Crustacea into two subclasses, the *Neocarida* and *Palæocarida*, the latter group comprising the Merostomata and Trilobita. In a previous paper we had shown the close homologies of the eye of Trilobites to the compound eyes of Limulus.

In April, 1881, Mr. C. D. Walcott (Bull. Mus. Comp. Zool., viii, No. 90, p. 209), under the class Pœcilopoda, places two subclasses, viz, Merostomata and Palæadæ (Trilobita), giving definitions of the groups.

In 1881, in his article "Limulus an Arachnid" (Quart. Journ. Micr. Sc.) Prof. E. Ray Lankester proposed the term *Hæmatobranchia*, which he regarded as the equivalent of Merostomata. This group of the class Arachnida, as understood by Lankester, embraces the three orders: 1, Trilobita; 2, Eurypterida; and 3, Xiphosura.

In 1885 (Embryology of Limulus Polyphemus, III, Proc. Am. Phil. Soc., January, 1885), we referred Limulus, with the Eurypterida and Trilobita, to a class by themselves.

In 1885 Mr. J. S. Kingsley associated the Limulus with the Arachnids as a group by themselves, to which he gave the name *Acera* (Science News and Quart. Journ. Micr. Sc.).

In 1886, in the 5th edition of our Text-book of Zoology, we suggested the term *Podostomata* for the class comprising the two orders Merostomata and Trilobita.

IV.—*The class Podostomata.*

It thus appears that while at the present date (1886) A. Milne Edwards, E. Van Beneden, and E. R. Lancaster regard Limulus and its allied forms as belonging to the Arachnida, and J. S. Kingsley associates the Limulus and the Arachnida in a group by themselves under the name *Acera*, the present writer and Professor Claus regard the Merostomata with the Trilobites as forming a class intermediate between the Arachnida and Crustacea.

We have endeavored to show that the names Pœcilopoda and Gigantostracea have been applied in such different senses by different authors that they cannot well be retained for the Merostomata and Trilobita taken together in the sense we advocate. We have therefore proposed the term *Podostomata* for this class of Arthropoda. It is derived from πούς, ποδός, foot, and στόμα, mouth, in allusion to the foot-like or ambulatory nature of the cephalic appendages which surround the mouth in a manner characteristic of the group.

The class Podostomata may be defined as a group of Arthropods, in which the cephalic (Limulus) or cephalo-thoracic (Trilobites) appendages are in the form of legs, *i. e.*, ambulatory appendages, usually ending in forceps, or large claws (chelæ), which in the sole living representative of the class are arranged in an incomplete circle around the mouth; the basal joint of each leg is spiny, so as to aid in the retention and partial mastication of the food. No functional antennæ, mandibles, or maxillæ. Eyes both compound and simple. Respiration by branchiæ attached to the abdominal appendages, which are broad and lamellate in Merostomata and probably cylindrical with narrow gills in Trilobita. The brain (procerebrum) supplying nerves to the eyes alone; the nerves to the cephalic or cephalo-thoracic appendages originating from an œsophageal ring; the ventral cord ensheathed by a ventral arterial system more perfectly developed than in insects or scorpions; coxal glands highly developed, with no external opening in the adult. This class differs from the Arachnida, among other characters, in having no functional cheliceres ("mandibles") or pedipalps ("maxillæ"); in the cephalic appendages either ending in large claws or forceps, or simple, the terminal joint not bearing a pair of minute claws or ungues like those of Arachnida and Insecta, enabling their possessors to climb as well as walk. Podostomata have no urinary tubes. Limulus undergoes a slight metamorphosis, while in Trilobites the adult differs from the larva in having a greater number of thoracic segments.

From the Crustacea the Podostomata differ in the lack of functional antennæ and mouth parts; in the brain innervating the eyes (compound and simple) alone; in the shape of the head and of the pygidium or abdominal shield, and in the arterial coat enveloping the ventral nervous cord.

The Podostomata are divided into two orders:

I. *Merostomata* with three suborders, *Xiphosura*, *Synziphosura*, and *Eurypterida*.

II. *Trilobita*.

Explanation of Plate V.

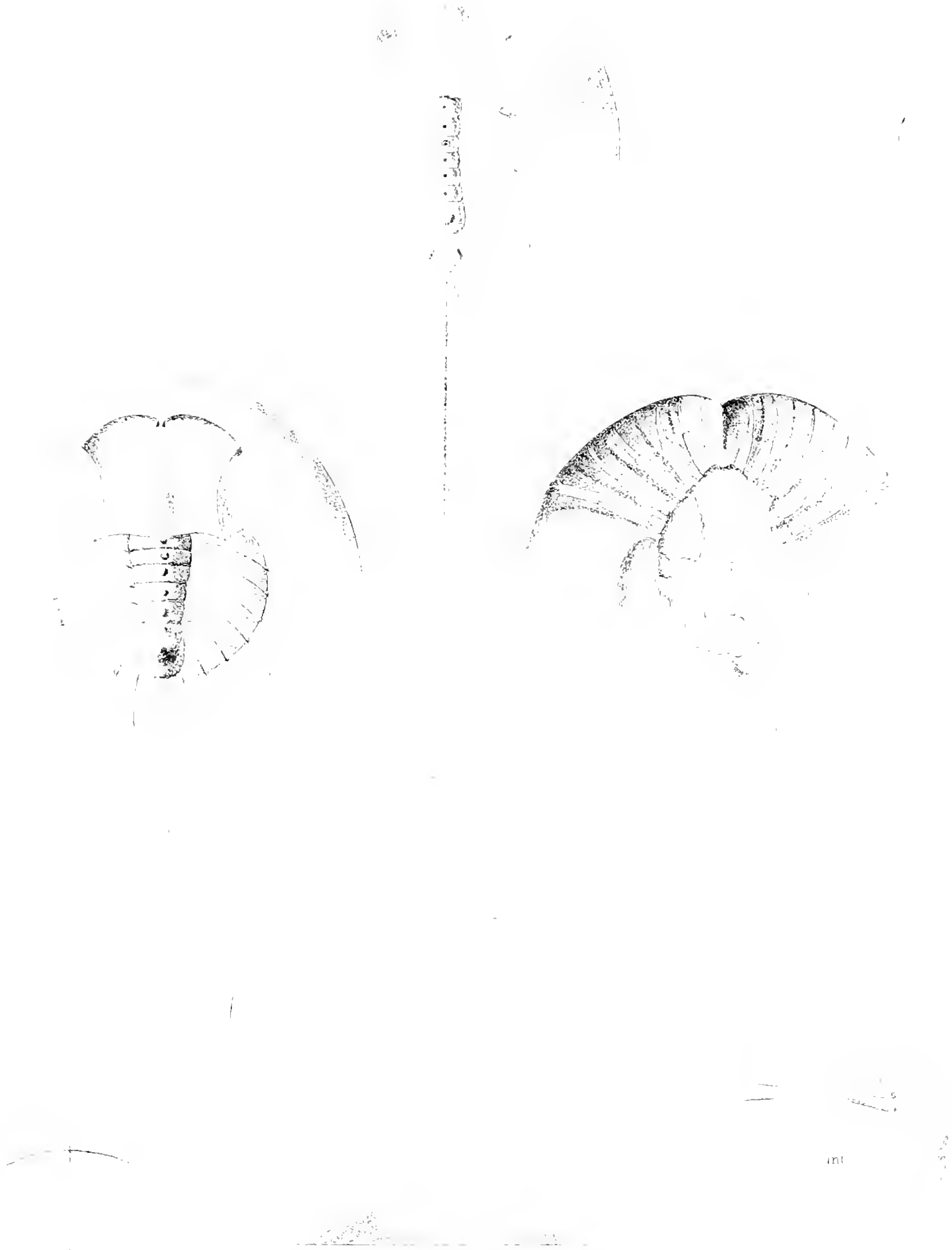
Fig. 1. *Cyclus americanus* Pack. $\times\frac{3}{1}$. 1*a*, lateral view restored. $\times\frac{3}{1}$.
Fig. 2. *Dipeltis diplodiscus* Pack. Natural size; 2*a*, the same restored. $\times\frac{2}{1}$.
Fig. 3. *Prestwichia danæ* Meek. Natural size; restored; dorsal view.
Fig. 3*a*. *Prestwichia danæ* Meek. Natural size; partly restored; ventral view.
Fig. 4. *Prestwichia longispina* Pack. Partly restored. $\times\frac{2}{1}$.
Fig. 5. *Belinurus lacoei* Pack. Partly restored. $\times 2$.
All the figures on this plate drawn by Dr. J. S. Kingsley, with corrections by the author.

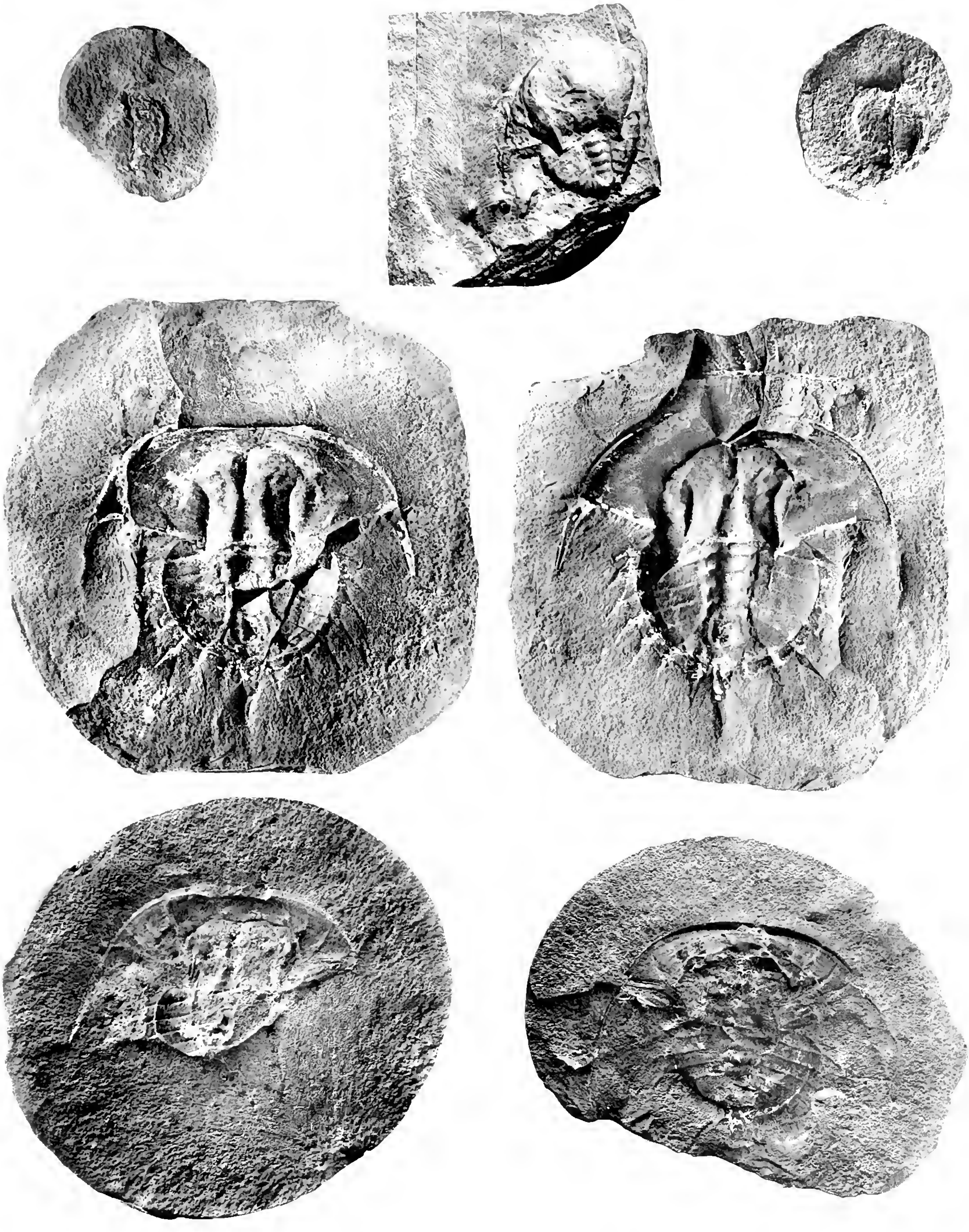
Explanation of Plate VI.

Fig. 1. *Prestwichia danæ*, showing the limbs; 1*a*, the reverse.
Fig. 2. *Prestwichia danæ*, showing the interior; 2*a*, the same, another specimen.
Fig. 3. *Prestwichia longispina*, natural size.
Fig. 4. *Cyclus americana*, natural size; 4*a*, reverse of the same.
From photographs taken by Mr. R. L. P. Mason, Providence, R. I.

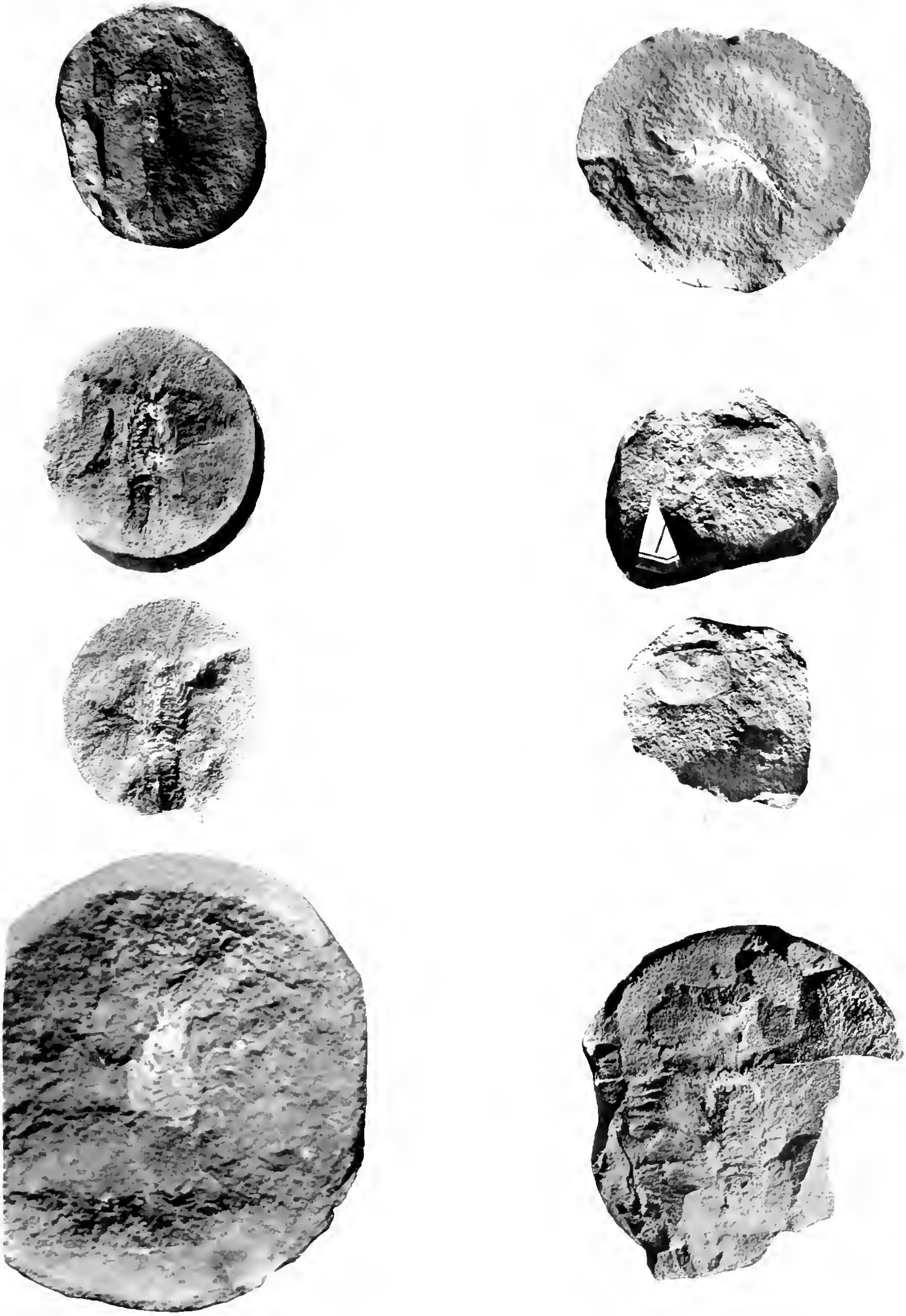
Explanation of Plate VII.

Fig. 1. *Palæocaris typus* M. & W., natural size.
Fig. 2. *Palæocaris typus* M. & W., natural size.
Fig. 3. *Anthrapalæmon gracilis* M. & W., carapace laterally flattened.
Fig. 4. *Anthrapalæmon gracilis* M. & W., from a small specimen without the carapace.
Fig. 5. *Anthrapalæmon gracilis* M. & W., carapace wanting.
Fig. 6. *Anthrapalæmon gracilis* M. & W., carapace wanting.
All the figures of natural size and from photographs taken by Mr. F. O. Draper, Pawtucket, R. I.





FIGS. 1-2. PRESTWICHIA DANÆ. 3. P. LONGISPINA. 4. CYCLUS AMERICANUS.



FIGS 1–2 PALÆOCARIS TYPUS 3–6 ANTHRAPALÆMON GRACILIS

NATIONAL ACADEMY OF SCIENCES.

VOL. III.

SEVENTEENTH MEMOIR.

ON TWO NEW FORMS OF POLYODONT AND GONORHYNCHID FISHES FROM THE EOCENE OF THE ROCKY MOUNTAINS.

ON TWO NEW FORMS OF POLYODONT AND GONORHYNCHID FISHES FROM THE EOCENE OF THE ROCKY MOUNTAINS.

READ NOVEMBER 12, 1885.

By E. D. Cope.

CROSSOPHOLIS MAGNICAUDATUS Cope, American Naturalist, 1883, p. 1152;* loc. cit., 1885, p. 1090.

Since the description of the part of the body of this fish was published, I have obtained a considerable portion of the skull of an individual of the same genus, if not species. It belongs apparently to a larger individual than the one first described. Although it was found at the same locality as the latter, and at near the same time, it is not part of the same individual. The characters of the family *Polyodontidæ* are easily discerned in the skull, and those of the body coincide.

The skull displays typical ordinal and family characters. The postemporal bone is produced backwards, and is separated from the parietal by a large foramen, which continues between the dermosphenotic and parietal. It is bounded in front by a process of the dermosphenotic, which joins a corresponding one from the frontal. Anterior to this connecting bridge, another large foramen extends, separating the dermosphenotic and an element continuous with it anteriorly, perhaps a branch of the nasal, from the frontal. The maxillary and mandibular bones occupy their usual position; and the hyomandibular extends posteriorly from near the junction of the pterotic and the epiotic. The epiclavicle† is simple, and the operculum arises from near the distal end of the hyomandibular. Immediately below its anterior portion is a flat element, which does not occur apparently in Polyodon. It may be preoperculum or suboperculum, or the interoperculum of Polyodon in a different position.

Interneural and interhæmal basilar bones are large and are ossified, and the caudal hæmapophyses ("hypurals") are but little less so. The caudal fin is entirely heterocercal. Vertebral axis consisting of notochord only.

Char. gen.—The bone in the position of preoperculum above mentioned, is in contact with the inferior face of the supero-anterior extremity of the operculum by its superior extremity. Its anterior border is entire, subvertical, and convex. Its posterior edge is not preserved, but the impression of its external surface is deeply grooved, as though the bone were ridged anteroposteriorly. There are preserved several loose thin plates of bone covered with small teeth *en brosse*, which have come from the premaxillary and dentary bones. There are preserved a number of stellate bones of the muzzle.

Scales numerous, in oblique series, not in contact, formed of a small grooved disk and several posterior spines. Dorsal and anal fins short, posterior; the former commencing in advance of the latter. Caudal fulcra posteriorly slender, continued into broad flat scuta extending forwards on the median line of the caudal peduncle. Superior lobe of caudal fin much more produced than inferior. A lateral line of tubules.

---

* By a typographical error, Crassopholis at this reference.

† I employ the nomenclature of Bridge in his paper on the Osteology of *Polyodon folium*, Philosoph. Transac. Royal Society, 1878, p. 688.

In the form of the muzzle and in the large caudal fulcra this genus resembles Psephurus rather than Polyodon. In its numerous slender fulcra it is, however, like the latter genus, while the anterior fulcra are more scutiform, and extend further forwards than in Psephurus. The problematical opercular bone differs from anything seen in Polyodon, even if it should prove to be a suboperculum, which is present in that genus. It is, however, separate from and below the operculum in that genus, and is separated from the cartilaginous ? quadrate by a cartilaginous peduncle.

The identification of the true affinities of this form is an important step in the history of the Chondrostei. Hitherto our knowledge of the Polyodontidæ has been restricted to the two recent genera named, and their relations to the ancient world have been unknown. We can now date the history of the family from the early Eocene period.

Char. specif.—On comparing this fish with the *Polyodon folium*, various differences appear besides those already referred to. The bones of the skull proper are as large as those of a middle-sized individual of the recent species named, while the muzzle is considerably shorter than would be found on such individual. Although the muzzle has been pressed obliquely, the sizes of its axial lenticular elements indicate that but little of its length is wanting. There is not much indication of the free lateral edges characteristic of the *Polyodon folium*, although the stellate bones are visible near the edge and at the base.

To commence at the posterior extremity of the skull: The epiclavicle is more robust than in the *Polyodon folium.* Its superior extremity is openly notched on the anterior edge, probably to fit a corresponding surface of the dermosphenotic. Its external face has a narrow, oblique ridge commencing opposite this notch and extending diagonally across to the posterior edge, with a gradual inclination. It forms the posterior edge of a groove which widens below and narrows above, reaching to within a centimeter of the articular notch. A similar groove exists in the paddle-fish, but it only extends as high up as the line of the superior border of the operculum. The posttemporal is shorter and more robust than in the paddle fish, and has the same wide, bridge-like connection with the parietal. Its external border is in a straight line with that of the dermosphenotic. The parietal bones are lost from this specimen. The dermosphenotic is a more slender bone than in the paddle-fish, and the foramen separating it from the frontal bone is shorter and wider. It sends down a postorbital process, which is much like that of the paddle-fish, but is narrower, and ends in three tooth-like processes. There is a small preorbital process, and below it a palmate ossicle, which may have been broken from it or may be distinct. Shortly anterior to this, the element terminates in a laciniate suture, with a narrow, straight band, which is continuous as a posterior divergent branch of the possible nasal bone. The junction of the latter with the frontal closes the foramen above described in front. The bone exterior to this band, which occupies the place of a prefrontal in the *Polyodon gladius*, is either wanting or is represented by a spiculum which has become separated. The muzzle presents several of the lenticular spicular bones seen in the living species. Some stellate bones lie about the base of the muzzle, out of place, and a band of them lies on the slab at a short distance on one side. These are characterized by smaller size and more slender radii than in the paddle-fish.

The premaxillary bone is gently convex upwards, instead of straight as in the *P. folium.* The dentaries have a corresponding form. They are widely and deeply grooved on the inner side for Meckel's cartilage, which is covered by a small subtriangular bone (? splenial) at the symphysis. This element is very much smaller than in the paddle-fish. The alveolar edge of the dentary bone is acute, and bounds a rabbet whose floor is the superior roof of Meckel's cartilage. The hyomandibular bone is a robust, flattened rod, and is apparently curved so as to be convex forwards, although this appearance may be due to injury of the specimen. It is in any case shorter than in the paddle-fish, where it is also straight. The operculum is relatively and absolutely larger than in the *P. folium.* Its anterior inferior border is concave, and is thickened on the descending portion. Its anterior extremity is beveled on the external side for attachment with an element already mentioned, which may be preoperculum. The greater part of the latter is lost.

Several dentigerous laminæ lie among the jaw bones, from which they have been separated. They are concave on the inferior side, so as to embrace the alveolar borders, probably of the premax-

illary bones, as those of the dentaries are too acute. Determinable palatine bones are not visible in the specimen. The bases of the teeth are round and are close together. They measure .066^{mm}.

The body of the originally described individual presents the following characters:

Interneural basilar bones 16 to 18; body slender at dorsal fin and contracted at caudal peduncle. Dorsal and anal fins moderate, caudal very large, with strong anterior fulcral rays. The posterior fulcra are elongate and subcylindric, and overlap each other extensively. Anteriorly they flatten so as to approach the form seen in the paddle-fish, but these are well distinguished from the fulcral scuta in front of them. The latter are longitudinally oval, and have a groove along the middle line. The caudal hæmapophyses are short and expanded distally. They soon disappear in the superior lobe or axis of the caudal fin. I count nine to the anterior more or less cylindric ones. At the base of the inferior lobe of the caudal fin are five fulcra lying on each other, the inferior ones more flattened than the superior ones, all with acute posterior apices.

The scales are subquadrate in form and measure about a millimeter each way, including the spines; they are nowhere in contact and are more widely separated anterior to the dorsal fin than posterior to it. The sides of the axis of the superior lobe of the caudal fin are covered with closely packed oat-shaped scales.

Length from notch of caudal fin to line of anterior base of dorsal, M. .170; depth at anterior base of anal, .060; depth of caudal peduncle, .035; length of inferior lobe of caudal, .110. Length of an interneural basilar, M. .012; of an interhæmal basilar, .014. Probable number of dorsal radii, 24. Anal fin imperfect.

NOTOGONEUS OSCULUS Cope, American Naturalist, Nov., 1885, p. 1091.

Family char.—The location of this genus is rendered quite possible by the excellent preservation of two specimens of the typical species which have come into my possession. The form is plainly isospondylous, and belongs to a family in which the parietal bones are separated by the supraoccipital;* the superior border of the mouth is formed by the premaxillary bone exclusively; the pterotic and intercalary bones are normal; the caudal fin is homocercal, and the dorsal and anal fins posterior, and with few radii. There is no indication of adipose fin under most favorable circumstances for its preservation, and the branchiostegal rays are three or four. These characters place the genus within the limits of the family Gonorhynchidæ, of which but a single genus and species have been hitherto known. This fish is now living in the waters of the Cape Colony of South Africa, and of South and West Australia, and, it is said, also in those of Japan. The discovery of this type in the Eocene beds of North America is a notable addition to ichthyological science. It is parallel with the occurrence of the family of the Osteoglossidæ in the same formation, a family also now confined to the Southern Hemisphere. It will be seen on comparing the generic characters that this genus is very nearly allied to the living one.

Char. gen.—Body covered with scales whose borders have a fringe of rather long spines. Mouth small, probably a barbel on each upper lip, as a spicule of bone projects downwards and backwards from each side of the end of the muzzle. No traces of teeth in the jaws or on the pterygoid or hyoid bones. An oval body at the superior extremity of the posterior branchihyal arches below the vertebræ. Caudal fin bifurcate.

In the above diagnosis the only character which separates this genus from Gonorhynchus is the absence of the dental apparatus of the hyoid and pterygoid bones which characterizes the latter. It is probable that if the suprabranchial mass above referred to be the homologue of the similar organ in Gonorhynchus, there is here also an important difference. In Notogoneus this is sacciform. In Gonorhynchus it is lamina. The muzzle of the fossil species is not so prominent as in the recent one, but this being a matter of proportion only, may be only a specific character. The addition of a new genus to a family hitherto so little represented is a circumstance of interest, but not entirely unexpected. It is in generalized families like this one and the Galaxiidæ, that we are to look for additions in the faunæ of the early Tertiary and Mesozoic periods.

Some characters which do not enter the specific category may be here referred to. The maxillary bone is bordered on its entire anterior edge by the premaxillary, and has no supplementary

* I find on renewed examination of the *Gonorhynchus greyi* that the frontal bones extend far backwards, so that parietals are entirely separated by the supraoccipital.

bones. The mandibular ramus is very deep at the coronoid region, and if the fissures are not fractures, the angular bone is distinct. The frontal bones are extended well forward to a point above the mouth. The probable ethmoid in front of them is short and wide. The frontals extend also well posteriorly, meeting the supraoccipital. The superior face of the latter is rather large and is subtriangular in outline and has a produced nuchal crest. The intercalaries each present a sharp angle posteriorly. The branchiostegal rays are robust. No entire vertebra included in the caudal fin, but the usual modified one.

Char. specif.—The general form is elongate, and the head is short. The dorsal and ventral fins, which are opposite each other, are posterior in position, while the short anal is not far removed from the caudal. The lobes of the latter are long and divergent. The muzzle overhangs the mouth a little, but the latter is so short that the end of the premaxillary bone only reaches half way to the anterior border of the orbit. The operculum is convex downwards and posteriorly. The length of the head enters the length with the caudal fin six and a half times; and the greatest depth, which is in front of the dorsal fin, enters the same six and three-quarters times. Vertebræ: abdominal, 34; caudal, 14½. Radii: Br. III or IV, robust; D. I (very short), 12; A. II (very small), 8; V. (very small), 6 or 7; P., not countable. The scales are of medium size, five longitudinal series in a centimeter anterior to the ventral fin. Their surface is sculptured by longitudinal, parallel, fine, sharp grooves of .03^{mm} in width. The fringe on the free edge of the scale is composed of flat, acute, and rather long, closely-set spines.

Measurements.

Total length	.485
Length to base of caudal fin	.395
Length to line of anal	.332
Length to line of dorsal	.224
Length of head	.081
Length of gape of mouth	.015
Width of skull at orbits	.019
Width of skull at pterotics	.030
Depth at front of dorsal fin	.070
Depth at front of anal fin	.050

From Twin Creek, Wyoming Territory.

PRISCACARA HYPSACANTHUS sp. nov.

The specimens of this species which have come under my observation are the smallest of the genus, and I have therefore questioned their maturity. They have, however, all the characters of adults, and as I have now seen a number of specimens which agree in various peculiarities as well as in size, I am satisfied as to their representing a species which has not as yet been recorded.

The *Priscacara hypsacanthus* belongs to the section of the genus with a small number of soft dorsal rays, and with robust ventral spine. It differs from all the species in the length of its slender dorsal spines, especially the fourth, fifth, and sixth. The superior outline of the head and body are but little arched from a straight line. The form is moderately robust. The scales are small. Length of head equal to greatest depth, *i. e.*, at front of dorsal fin, and entering length with caudal fin, 3.4 times. Orbit large, entering length of head four and one-half times. The origin of the dorsal fin is a little anterior to that of the ventral. Its superior outline is notched to two-fifths the length of the spine of the second dorsal. The border of the caudal fin is a little concave. The first anal spine originates below the third soft ray of the dorsal, and the soft rays of the ventral reach the same point. Vertebræ: Abd. X; caud. XIV. Rays: Br. VII; D. X, 9; the last soft ray split into two: A. III, 9. Scales in eight rows in an oblique band from the last soft ray of the anal fin to the vertebral column. Total length, M. .062; to base of caudal fin, .049; to line of first anal ray, 0.33; to line of base of ventral, .023; of head, .0168; depth at ventral fin, .016; at last ray of dorsal, .011; at base of caudal, .007. The specimens are from Twin Creek, Wyoming Territory.

Besides other peculiarities, the presence of an elongate spinous ray at the front of the second

dorsal fin distinguishes this species from all the others of the genus. The number of spinous rays is the same as in them.

I may add that a newly acquired specimen of the *Priscacara serrata* displays the massive superior and inferior pharyngeal bones, covered with obtuse grinding teeth.

EXPLANATION OF PLATE.

FIGS. 1-3. *Crossopholis magnicaudatus* Cope, one-half natural size, except fig. 3, which is magnified four diameters.

FIG. 1. One side of skull lacking the parietal bone, and other elements more or less disarranged; from the left side. *Na* nasal bone; *Fr.*, frontal; *Dspo.*, Dermosphenotic; *Pa.*, parietal; *Pot.*, post-temporal; *Ecl.*, Epiclavicle; *Prmx.*, premaxillary; *D.*, Dentaries; *Hm.*, Hyomandibular; *Op.*, operculum; *P.*, problematical element; *St.*, stellate bones; *Dp.*, Dentigerous laminæ.

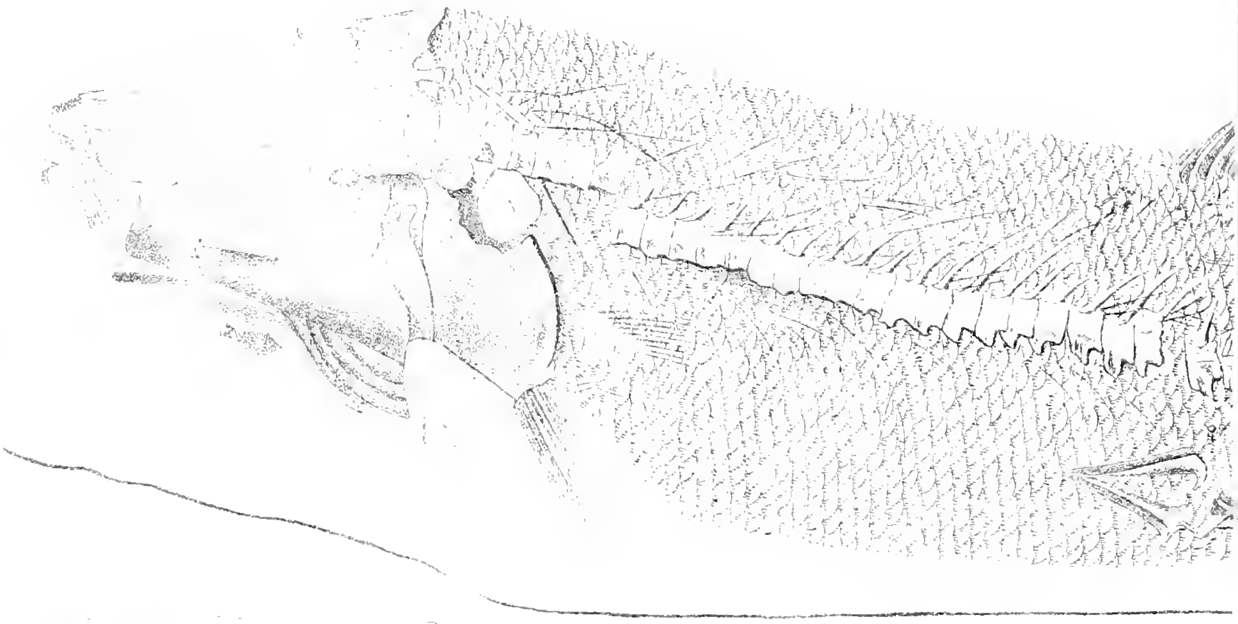
FIG. 2. Part of the left side of the body, lacking a piece of the caudal peduncle. *Nb.*, neural basilars; *H b.*, hæmal basilars; *Ch.*, caudal hæmal spines ("hypurals").

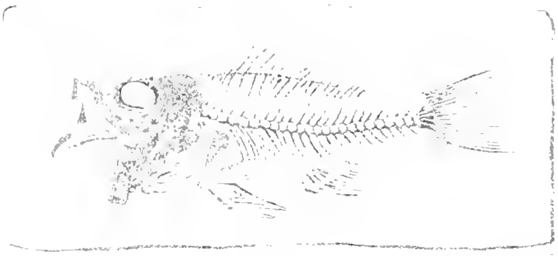
FIG. 3. Scales of the same magnified four diameters.

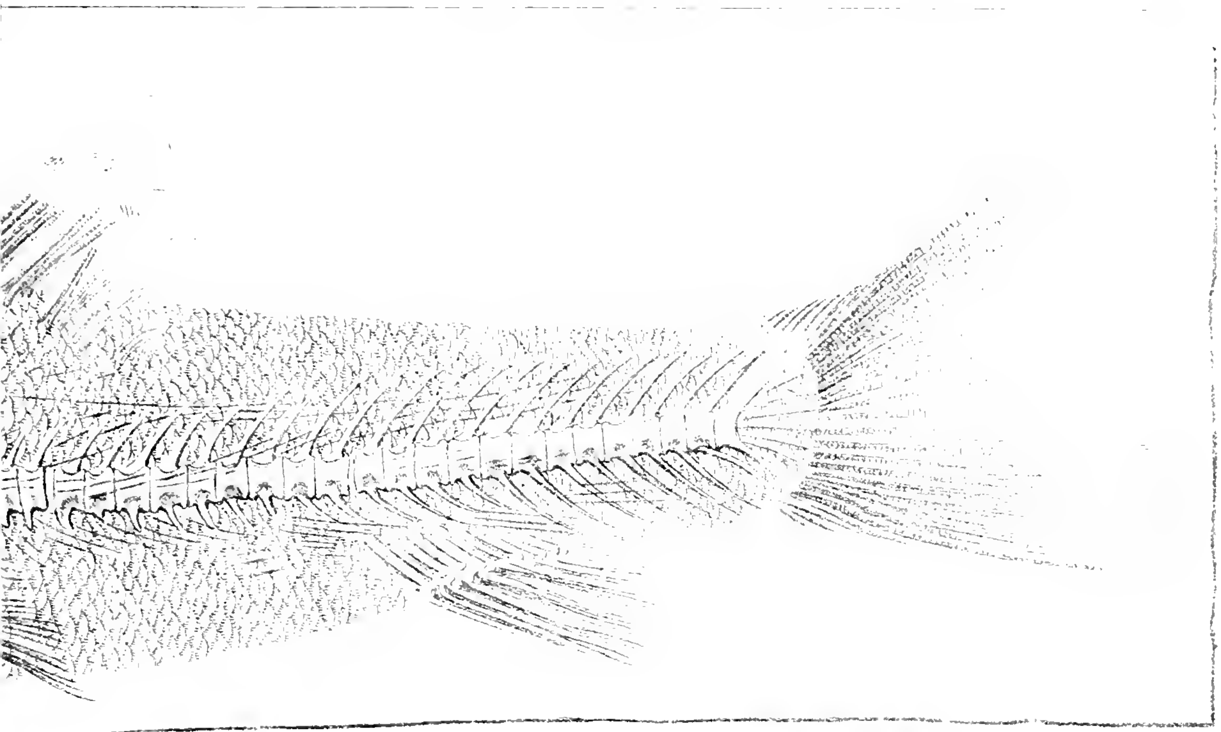
FIG. 4. *Notogoneus osculus* Cope, two-thirds natural size; *A.*, accessory ? branchial organ; *B.*, ? barbel axis.

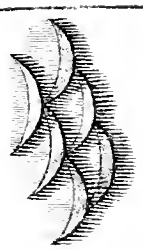
FIG. 5. Scales of do., natural size.

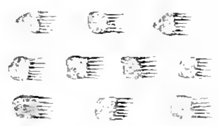
FIG. 6. *Priscacara hypsacanthus* Cope, natural size.











III

II

NOTES ON THE THIRD MEMOIR, PAGE 45.

By ALFRED M. MAYER.

RÉSUMÉ OF BIBLIOGRAPHY PERTAINING TO THE PAPER "ON A METHOD OF PRECISELY MEASURING THE VIBRATORY PERIODS OF TUNING-FORKS, ETC."

Since the publication of the above memoir I have been enabled to look into publications of learned societies of Holland and Germany, heretofore inaccessible to me, and deem it proper to give the results of those examinations, as it allows me to render credit to others who have anticipated me in methods described in my paper, and which I thought I was the first to originate. These methods I have, however, developed by my researches into a degree of precision not attained by those investigators who have anticipated me. The paper of Beetz in Pogg. Ann. I overlooked, although that journal is in my possession, and had previously been examined for investigations allied to those of my memoir.

Apart from the pure pleasure afforded by original investigation, the only reward the man of science has is the credit given him by his fellow-workers in the same lines of research, and as I, like other Americans, have had the experience of seeing my endeavors to advance science overlooked, I know of the injustice thus done—we hope inadvertently—by those who should at least have taken the trouble of searching in accessible journals the cognate work of other investigators before publishing their own.

Donders, 1865, *in Nederl. Archief. voor Genus en Naturkunde*, II, page 332, discovered that the spark of the secondary circuit of the inductorium is composed of several separate sparks, and that one spark is obtained only when the striking distance in the secondary circuit is great. He used a slowly revolving mirror in the same manner as was used by Feddersen (Pogg. Ann., CXIII) in his observations on the electric spark of the Leyden jar.

Donders, 1868, *Onderzokingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschool*, II, pages 316–318. He sent the spark of the inductorium from a metallic style attached to the prong of a vibrating fork. The style made its trace on the surface of smoked paper, and the number of sparks in the discharge and the duration of the discharge were given in the sinuous trace of the vibrating fork.

Nyland, 1870, *Archives Neerlandaises*, pages 292–337, with ten photographic prints of experiments. Nyland, at the request of Donders, continued the latter's experiments of 1868. He shows that the number of sparks in the discharge diminish with current of battery and with the increase of striking distance in the secondary circuit; shows the effect of increasing the resistance of the medium through which the secondary spark passes; also, that the duration of the discharge does not diminish in the same ratio as the resistance to its passage. He obtained a parabolic curve by making the resistance abscissas and the durations of discharges the ordinates. On placing a Leyden jar in the secondary circuit and passing the discharge between the points of two styles which were placed near each other, and with those points on a line parallel to the axis of rotation of the cylinder covered with smoked paper, he obtained traces which showed an oscillatory action or to-and-fro discharge between the points, similar to the figures obtained by Feddersen (Pogg. Ann., 1862) on a photographic plate, which received from a revolving concave mirror the image of the discharge of a Leyden jar charged with static electricity. Of these traces Nyland says: It is

certain that these images yield nothing to those of Feddersen in fineness of detail. Nyland in this paper also first describes the method of obtaining the vibratory period of a fork by passing the secondary spark of an inductorium from a style, fastened to the fork, to the smoked paper covering a revolving metallic cylinder. The primary circuit was closed and opened each second by a clock. He, however, did not experiment to bring this method to give precise results.

Helmholtz, 1869, *Verhandlungen des naturhistorischen medizinischen Vereins zu Heidelberg*, observed that the discharge of an inductorium, with a Leyden jar in the secondary circuit, into the nerve of a frog caused 45 maxima and minima of contractions, but that these vibratory phenomena were not observed when the Leyden jar was absent.

Rood, 1872, *American Journal of Science*, observed the multiple character of the discharge of an inductorium, with a Leyden jar in the secondary circuit, by means of a revolving mirror and a rotating disk. The revolving disk was formed of two superposed disks, each with a radial slit. By rotating the smaller disk an angular separation of the slits could be obtained, so that when the reflection of the discharge from white paper was viewed through these slits the images of the multiple slits could be brought together, so that the end of one set of images given by one slit just touched the set of images given by the other slit. Knowing the velocity of rotation of the disk and the angle separating its two slits, the duration of the composite discharge was obtained. He thus observed as many as 10 to 20 separate sparks in a discharge (with jar of 114 square inches of surface in circuit) whose duration was about $\frac{1}{100}$ of a second. With platinum points as electrodes, and separated by 1, 2, 3, 4, and 5 millimeters, the number of sparks observed at these distances of the electrodes were respectively, 4, 3, 2, 1, and 1. With a jar of only 11 square inches of surface, and the electrodes formed of brass balls, the discharge was more complex. Observing in the revolving mirror he saw the discharge formed of a bright spark followed by a violet discharge, and the latter followed by four sparks. The total duration of this discharge was $\frac{1}{50}$ of a second. The violet portion lasted $\frac{17}{1000}$ of a second, and the four sparks lasted $\frac{2}{1000}$ of a second. Other forms were sometimes present, consisting of a faint violet streak terminated at each end by a spark, the whole duration being $\frac{1}{35}$ of a second. On increasing the striking distance between the balls to 2, 3, 4, 5, 6, 7, 9, and 10 millimeters, the total number of sparks forming the discharge for the striking distances was, respectively, 5, 8, 4, 3, 3, 3, 2, and 1 spark. In the series of papers (1869–'72) containing the above results, Rood, by novel, refined, and precise methods, first succeeded in obtaining the duration of one of the separate sparks which go to make up the number forming the composite discharge. This he did by examining through a microscope a series of fine rulings on smoked glass of bright and dark bands of equal breadths when illuminated by the discharge. These lines were so fine that the magnified image of one of these measured $\frac{1}{300}$ of an inch. When viewed in the revolving mirror they appeared as they did when the mirror was stationary, till the mirror approached 180 revolutions in a second. Then the lines grew fainter and fainter, and, finally, when the mirror reached 183 revolutions in a second the lines disappeared by the mirror making the reflection of a black band of the ruling to be displaced to its own width during the duration of the spark. Rood thus found that the duration of this portion of the discharge was .000000175 of a second. With a jar of only 11 square inches of surface in circuit, and by using finer rulings and a more rapidly revolving mirror he determined that the spark lasted only forty-eight billionths of a second. "With this light" (lasting only forty billionths of a second) "distinct vision is possible. Thus, for example, the letters on a printed page are plainly to be seen. Also, if a polariscope be used, the cross and rings around the axis of crystals can be observed, with all their peculiarities, and errors in the azimuth of the analyzing prism noticed. * * * All of which is not so wonderful, if we accept the doctrines of the undulatory theory of light, for, according to it, in forty billionths of a second nearly two and a half millions of the undulations of light reach and act on the eye."

Cazin, 1873, *Journal de Physique*, observed the multiple character of the discharge of inductorium with an apparatus similar to Rood's.

Mayer, 1874, *American Journal of Science*, in this paper he refers to the previous work of Henry, Feddersen, Rood, and Cazin. He used a large inductorium, having a "striking distance" of 21 inches. With electrodes of platinum points, one millimeter apart, and no jar or condenser in circuit, he found the discharge of this inductorium to consist of 33 sparks, lasting $\frac{1}{23}$ of a second.

With a jar of 242 square centimeters in circuit, the discharge contained 91 sparks, and lasted $\frac{1}{24}$ of a second. The method used and results reached are described in his memoir in Vol. III, Nat. Acad. Sci.

Beetz, 1868, *Pogg. Ann.*, devised a chronoscope by flashing the sparks of a Leyden jar charged with static electricity from the style of a tuning-fork drawn over a smoked surface of tin foil. He subsequently replaced the sheet of foil with a metallic cylinder covered with smoked paper. With this apparatus he measured the time of falling bodies, and obtained $\pm$.0015 of a second as the difference between the observed and computed times of fall.

Rice, 1875, made seven determinations of the velocity of fowling-piece shot. The shot was of numbers 2 and 7. With a charge of 3 drachms of powder and $1\frac{1}{4}$ of No. 7 shot he obtained a mean velocity, in a range of 50 yards, of 855 feet per second. He used in these experiments a Le Boulangé chronograph. Professor Rice, U. S. N., was, I believe, the first to determine accurately the velocity of bird-shot. His interesting paper is published in "Rod and Gun," July 31, 1875.

Errata.

In memoir "On a Method of Precisely Measuring the Vibratory Periods of Tuning-Forks," &c., Vol. III, Pt. II, of Mem. Nat. Acad. Sci., by Alfred M. Mayer, on pages 54, 55, and 56, wherever "Tuileries fork" occurs *read* Feydean fork, and wherever "Feydeau fork" occurs *read* Tuileries fork.